Essays on the German Labor Market

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Abstract

This thesis comprises three essays, out of which the first two study the phenomenon of worker mobility across occupations in the West German labor market. The first essay studies the causal wage effects of mobility across firms and occupations among graduates from apprenticeship training. Exploiting variation in regional labor market characteristics the instrumental variables estimations indicate that occupation switches within the training firm involve a career progression. For job switches the loss of firm-specific human capital seems to dominate. However, the wage loss does not grow when an occupation switch occurs simultaneously. In light of these results, the second essay in this thesis studies patterns of occupational mobility in West Germany over the period 1982–2008 separately within and across firms. Most importantly, occupational mobility rates across firms have significantly increased since the early 1980s, while within-firm occupational mobility rates have significantly decreased. The essay also assesses potential explanations for these developments, such as demographic change or the relationship between occupational mobility and unemployment. The third essay in this thesis studies the relationship between product market deregulation and labor market outcomes. It exploits the 2003 reform of the German Crafts Code as a natural experiment to study how the abolishment of barriers to firm entry may affect self-employment and dependent employment. Since there are doubts regarding the validity of the identifying assumptions, the results cannot be interpreted causally. Nevertheless, the analysis at least partially corroborates the evidence for a positive reform effect on self-employment documented elsewhere in the literature, while the reform seems not to have had a positive effect on dependent employment in the deregulated crafts occupations.

Keywords:
Labor economics, German labor market, Occupations, Occupational mobility, Wage effects of occupational mobility, Apprenticeship, Dual system of vocational training, Product market deregulation, Barriers to entry, Skilled crafts and trades, Self-employment, Employment

Zusammenfassung

Diese Dissertation umfasst drei Aufsätze, von denen sich die ersten beiden mit dem Phänomen der beruflichen Mobilität von Arbeitnehmern im westdeutschen Arbeitsmarkt befassen. Der erste Aufsatz untersucht für Absolventen einer dualen Berufsausbildung die kausalen Lohneffekte von Mobilität über Firmen und Berufe hinweg. Die Instrumentenvariablenschätzungen, welche exogene Variation in regionalen Arbeitsmarktcharakteristika ausnutzen, zeigen, dass Berufswechsel innerhalb des Ausbildungsbetriebs einen Karrierefortschritt darstellen. Bei Jobwechseln dominiert der Verlust von firmenspezifischem Humankapital. Allerdings nimmt der Lohnverlust nicht weiter zu, wenn zusätzlich zur Firma auch der Beruf gewechselt wird. Angesichts dieser Ergebnisse dokumentiert der zweite Aufsatz Muster von beruflicher Mobilität in Westdeutschland über den Zeitraum 1982–2008 innerhalb von und zwischen Firmen. Die Häufigkeit von beruflicher Mobilität hat seit 1982 zwischen Firmen signifikant zugenommen und innerhalb von Firmen signifikant abgenommen. Die Analyse betrachtet zudem mögliche Erklärungsansätze für diese Entwicklungen, wie zum Beispiel den demografischen Wandel oder den Zusammenhang zwischen beruflicher Mobilität und Arbeitslosigkeit. Der dritte Aufsatz untersucht den Zusammenhang zwischen Produktmarktderegulierung und Arbeitsmarktergebnissen. Die Reform der deutschen Handwerksordnung von 2003 wird als natürliches Experiment genutzt, um mögliche Einflüsse der Abschaffung von Markteintrittsbarrieren auf selbstständige und abhängige Beschäftigung zu untersuchen. Da Zweifel an der Gültigkeit der identifizierenden Annahmen aufkommen, können die Schätzergebnisse nicht kausal interpretiert werden. Dennoch legen die Ergebnisse zumindest in Teilen nahe, dass der in der Literatur bereits dokumentierte positive Effekt auf selbstständige Beschäftigung plausibel ist, wohingegen die Reform vermutlich keinen Anstieg der abhängigen Beschäftigung in den deregulierten Berufen zur Folge hatte.

Schlagwörter:
Arbeitsmarktökonomik, deutscher Arbeitsmarkt, Berufe, berufliche Mobilität, Lohneffekte beruflicher Mobilität, Berufsausbildung, Duales System der Berufsausbildung, Produktmarktderegulierung, Markteintrittsbarrieren, Handwerk, Selbstständigkeit, Beschäftigung

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the Labour Market" organized by the Priority Program 1764 in Mannheim. I particularly want to thank my colleagues at the Berlin Network of Labor Market Research, where I served as a board member for several years, for all the interesting discussions at seminars, workshops, and over dinner.

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Last but not least, I thank my family and friends who have accompanied me along the way towards finishing this dissertation. I am especially indebted to my parents and my husband. Without their continuous support this academic undertaking would have been much more difficult and certainly less quick. Admittedly, little Samuel hasn't made finishing this dissertation easier, but he has definitely made it more exciting.

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1 Introduction

Over the past centuries economists have discussed and repeatedly worried how technological progress affects employment and the future of work (Mokyr *et al.* (2015)). The tasked-based approach going back to Autor *et al.* (2003) contributes to this discussion by categorizing the content of jobs into manual and cognitive, as well as routine and non-routine tasks. This approach argues that while computerization substitutes for routine manual and routine cognitive tasks, it actually complements non-routine cognitive tasks. Accordingly, the impact of technological change on employment is heterogeneous: especially favoring non-routine cognitive tasks, while leading to a decline in the demand for routine-intensive labor. This has created a polarization of employment towards what Goos and Manning (2007) in their study on job polarization in the UK term "lovely and lousy jobs" – a move of relative labor demand away from the middle of the skill distribution towards non-routine manual (low-skilled) jobs and non-routine cognitive (high-skilled) jobs. Despite the understanding that technological progress may hurt parts of the workforce, in hindsight technological change has not led to large-scale technological unemployment during the past centuries (Mokyr *et al.* (2015)). However, the general perception of technological change has recently changed again. Over the past few years the debate has grown more fearful of a further shift in the relevance of production factors from labor towards capital, and of rising automation-induced unemployment and increasing income inequality, as has been accurately summarized by The Economist (2014). At the heart of this new, more widely-spread skepticism lies the realization that recent technological advances, such as in the areas of machine learning and robotics, have expanded the capabilities of software and machines in unexpected ways. In their widely cited book, Brynjolfsson and McAfee (2014) describe how within only a few years time information technology has made enormous progress regarding key features, such as pattern recognition and complex communication, which were long thought to mark the limits of automation potentials. The authors provide a wide range of examples that reflect these recent technological advances, such as the invention of autonomous vehicles, automatic language translation services, and very flexible general-purpose robots that are initially trained by taking hold of their wrist and guiding them through the required motions. Brynjolfsson and McAfee (2014) argue that, now that computer technology has achieved certain key capabilities, we are entering a "Second Machine Age" of accelerated technological progress which will fundamentally transform the economy. While the authors argue that the resulting transformation of the economy will be largely beneficial, they also predict that digitization will have disruptive effects on employment. In another widely discussed paper, Frey and

Osborne (2013) study how the computerization of non-routine manual and non-routine cognitive tasks, which have long been thought to be largely exempt from automation, could affect U.S. employment. Based on expert assessments they predict how likely certain occupations are to become automated. They find that those occupations which are at high risk of being computerized within the next two decades correspond to about 47% of total U.S. employment. Furthermore, they predict that computerization will especially substitute for low-skilled jobs. Apart from transportation and logistics, administrative and office jobs, as well as occupations in production, they predict that service occupations will also be strongly affected. Applying the occupation-specific computerization risks computed by Frey and Osborne (2013) to German data, Brzeski and Burk (2015) predict that in Germany about 59% of jobs are in danger of being automated.

However, recent empirical results suggest that the future of work may not be as dark as it has been painted by some over the past few years. In a recent paper, Arntz *et al.* (2016) argue that most occupations should contain at least some task-components, such as for example human face-to-face interactions, that are not easily automatable. They modify the approach underlying Frey and Osborne (2013) by taking account of the heterogeneity of task-profiles within occupations. Applying their approach to 21 OECD countries, their prediction of the job loss potential through computerization is much less extreme. For the U.S. they predict that about 9% of jobs are at high risk of being computerized. Their study also reveals a large heterogeneity of computerization shares across countries, that range from about 6% of jobs in Korea and Estonia to about 12% of jobs in Austria and Germany. Similar to Frey and Osborne (2013) the authors predict that low-skilled jobs will be most affected by automation. As Arntz *et al.* (2016) point out, their results for Germany are in line with the results of two other recent studies. While Dengler and Matthes (2015) follow a different approach to identifying the task-profiles of occupations, they find that about 15% of German employment that is subject to social security contributions is at high risk of being automated. In addition, recent employee survey data for Germany shows that on average about 13% of German employees think it likely that their job will be replaced by machines within the next decade (Bundesministerium für Arbeit und Soziales (2016)). This share is especially high for the group of low-skilled employees (28%). Furthermore, Arntz *et al.* (2016) emphasize that, while a significant share of jobs are at risk of computerization according to current predictions, this risk will not necessarily fully translate into actual employment losses. Firstly, new technologies may also create new demand for labor and, thus, new jobs. Secondly, workers may be flexible and adapt to the introduction of new technologies by providing new complementary skills. These very recent contributions therefore draw a less dramatic picture of the effects of technological change and the upcoming wave of automation on employment. However, even such more positive scenarios imply that workers will have to be occupationally flexible for years to come. They will need to continuously adapt to changing labor demands and new occupational requirements, and the pace at which these changes are required may become even faster in the future. It is therefore important to better understand

the potential of workers to be flexible regarding the type and the contents of their work, as well as workers' ability to switch occupations.

Over the past decade a small but growing strand of the economic literature has begun to investigate the occupational mobility behavior of workers with the aim of answering the following questions: Are workers occupationally flexible? If so, what individual labor market consequences does occupational mobility have? Most empirical studies on occupational mobility analyze the phenomenon based on individual-level data that contains information on workers' occupations according to a certain occupational classification. Occupational mobility is then defined as a change in the reported (or assigned) occupational code – with some studies applying further identification rules to increase the plausibility of the mobility variables. Admittedly, as Arntz *et al.* (2016) point out, occupational classifications provide a coarser measure of the contents of work than more detailed task-based measures do. Nevertheless, at an aggregate level the study of mobility across occupations can provide valuable insights into the occupational flexibility of workers. As Cortes (2016) shows, occupational mobility can serve as a means for workers to react to task-specific changes in labor demand that are induced by technological change. Furthermore, Berger and Frey (2015) predict that, as a reaction to the new wave of technological progress and automation, human workers will have to shift their professional activities towards more social and creative jobs. If that comes true, occupation-based measures of worker mobility may become even more relevant in the future.

Indeed, occupational mobility in the US and Western European economies is sizable (compare Section 2.1), and there is empirical evidence that workers have become more occupationally flexible over the past decades (Kambourov and Manovskii (2008); Moscarini and Thomsson (2007); Parrado *et al.* (2007); Seibert (2007); Lalé (2012)). While occupational mobility may allow workers to adjust to changes in labor demand, to the extent that the human capital obtained by workers through training and work experience is occupation-specific, it may involve a partial loss of human capital and subsequent wage losses (Kambourov and Manovskii (2009b); Gathmann and Schönberg (2010)). However, the economic literature also provides a second, more positive perspective on the individual labor market effects of occupational mobility: A switch of occupations does not necessarily have to be a perforce reaction, but it can also serve as a way for workers to actively advance their careers and realize wage gains (Fitzenberger and Spitz (2004); Fitzenberger and Kunze (2005); Longhi and Brynin (2010); Fitzenberger *et al.* (2015); Groes *et al.* (2015)). Given these differing views on occupational mobility and the heterogeneous labor market results of occupational mobility documented in empirical studies, further investigation of the causal effects of occupational mobility on individual labor market outcomes is required.

Against this background, Chaper 2 of my thesis contributes to the economic literature on occupational mobility by providing evidence on the causal wage effects of mobility amongst graduates from apprenticeship in Germany. In contrast to most other studies on occupational mobility, the analysis carefully distinguishs between two different dimensions of worker

mobility: mobility across firms, and mobility across occupations. Since selection into mobility may create biased results, my coauthors and I exploit variation in local labor market characteristics to implement an instrumental variables approach. Based on administrative data provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB) we find that pure firm changes and occupation-and-job changes after graduation from apprenticeship result in average wage losses, whereas occupational changes within the training firm result in persistent wage gains. We also allow for heterogeneous treatment effects with respect to the relative wage of the training occupation. Our results indicate that occupation switches within the training firm involve a career progression. In contrast to this, for job switches the loss of firm-specific human capital seems to dominate. However, the wage loss does not grow when an occupation switch occurs simultaneously. When we also take account of the direction of occupational switches, we find that a significant proportion of switches are directed towards generally better paid occupations. When firm-and-occupation switches are directed towards generally better paid occupations, the switchers even realize average short-term wage gains. Overall, our results suggest that the skills acquired through apprenticeship training in a certain occupation are, on average, sufficiently general to be of further use after a switch of occupation occurred.

The research project presented in Chapter 2 builds on my Diploma thesis, which I submitted to the University of Freiburg in 2010 under the title "Berufswechsel nach Abschluss der Berufsausbildung: Eine empirische Analyse auf Basis der IAB-Beschäftigtenstichprobe." In comparison to this earlier work, my coauthors and I have substantially modified and extended the scope of the analysis, the empirical design and the set of instrumental variables, as well as the estimation approach underlying our empirical study.

Chapter 3 of my thesis further contributes to the economic literature on occupational mobility by providing descriptive evidence on the extent and the evolution of occupational mobility in the West German labor market over the period 1982–2008. The empirical study contained in Chapter 3 thus adds to the small but growing number of studies that analyze country-specific patterns of occupational mobility, and it contributes to the discussion of whether workers have become more occupationally flexible over time. Since in Chapter 2 my coauthors and I observe significant differences between occupational mobility within and across firms, the analysis in Chapter 3 also differentiates between across-firm and within-firm occupational mobility. Based on administrative data provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB), I calculate yearly occupational mobility rates and show how they have evolved over time. Most importantly, I find that occupational mobility rates across firms have significantly increased since the early 1980s, while within-firm occupational mobility rates have significantly decreased. This chapter of my thesis also assesses potential explanations for these developments, such as demographic change, changes in the occupational and the industry structure of employment, as well as the relation between occupational mobility and career episodes that are marked by unemployment. Most of the findings differ substantially

between across- and within-firm occupational mobility, which suggests that different factors drive the decisions for across- and within-firm occupational mobility, respectively.

While Chapters 2 and 3 of my thesis both contribute to the literature on occupational mobility, the empirical study contained in Chapter 4 of my thesis falls into a different area of empirical labor economics. Chapter 4 contributes to the literature on the interdependencies between product market regulation and labor market outcomes. Over the past two decades, developed economies all around the world have witnessed a substantial easing of both economy-wide and, even more importantly, sector-specific product market regulations (Conway *et al.* (2005); Wölfl *et al.* (2009); OECD (2014)). Such regulatory changes are thought to be related to increased productivity and economic growth at the country level (e.g. Nicoletti and Scarpetta (2003); Djankov *et al.* (2006); Schiantarelli (2008)). At the same time, economists have become increasingly aware of the role product market regulations may play in explaining cross-country differences in labor market outcomes such as the evolution of employment. Economic theory predicts that the lowering of barriers to firm entry should lead to an increase in competition, and to a decrease in rents and, thus, the bargaining power of workers. This should have a positive effect on entrepreneurship and employment at least in the long run (Fonseca *et al.* (2001); Blanchard and Giavazzi (2003); Spector (2004)). Cross-country comparison studies indeed document a positive relationship between the ease of product market regulations, firm entry, and employment (compare Section 4.1). However, these studies also reveal a strong link between the strictness of product market regulation and country-specific characteristics such as the form of government, the extent of social and legal standards, or the extent of corruption and the shadow economy (e.g. Djankov *et al.* (2002); Dreher and Gassebner (2013)). Given the doubts this casts on the unbiasedness of cross-country results, a more recent second strand of the economic literature focuses on the evaluation of product market reforms within single countries to provide causal evidence on the link between the lowering of barriers to firm entry and labor market outcomes.

Chaper 4 contributes to this second strand of the literature by providing an empirical analysis of the labor market effects of deregulation of firm entry into the German skilled crafts and trades. It exploits the 2003 reform of the German Crafts Code as a natural experiment to study how the abolishment of barriers to firm entry may affect self-employment and dependent employment. The analysis uses data for the period 2000–2008 provided by the German Confederation of Skilled Crafts (ZDH), survey data provided by the Research Data Centers of the German Federal Statistical Office and the Statistical Offices of the German Federal States, as well as administrative data provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB). Using a differences-in-differences design, my coauthor and I investigate the link between the lowering of barriers to firm entry into crafts occupations and the number of registered crafts establishments, self-employment, as well as dependent employment in the German skilled crafts and trades. After the reform, the number of registered crafts establishments as well as the number of self-employed craftsmen increased relatively more

strongly in the deregulated occupations. The opposite holds for the number of dependently employed craftsmen. Individual-level regressions suggest a positive effect on the probability of being newly self-employed among all self-employed, and a negative effect on dependent employment probabilities. Unfortunately, since a close investigation of the institutional background and extensive empirical checks lead to doubts regarding the validity of the identifying assumptions, we cannot interpret the empirical results causally. Nevertheless, our estimation results at least partially corroborate the evidence for a positive reform effect on entrepreneurship and self-employment already documented elsewhere in the literature, while the reform seems not to have had a positive effect on dependent employment in the deregulated crafts occupations.

All three subsequent chapters are self-contained and can be read independently. Chapter 2 is joint work with Bernd Fitzenberger and Stefanie Lickleder and has been published as a research article in the peer-reviewed journal *Labour Economics* in 2015. Chapter 4 is coauthored with Alexandra Spitz-Oener.

2 Mobility Across Firms and Occupations Among Graduates from Apprenticeship

Legal Information

Please note that this chapter of my thesis has been published as a research article in Labour Economics:

Fitzenberger, B., Licklederer, S. and Zwiener, H. (2015) Mobility across firms and occupations among graduates from apprenticeship, *Labour Economics*, **34**, 138–151, doi:10.1016/j.labeco.2015.03.008.

You can access the online version of the published research article at: http://dx.doi.org/10.1016/j.labeco.2015.03.008.

In comparison to the research article published in Labour Economics, the text in Chapter 2 has been reformatted to fit the overall style of the dissertation. I have also updated some of the references, and I have integrated the research article's appendices, including the additional online appendix, into the dissertation's appendix.

2.1 Introduction

A large literature has documented sizeable mobility across firms and occupations in the US and Western European labor markets.[1] During the time period of 1979–2006 monthly occupational mobility rates in the US were at about 3.5% of overall employment – even higher than the 3.2% average rate of job mobility across firms (Moscarini and Thomsson (2007)). For Denmark, Groes *et al.* (2015) report that the annual occupational mobility rate lies close to 20%. While a large literature emphasizes the loss of firm-specific or occupation-specific human capital (e.g. Kambourov and Manovskii (2008); Gathmann and Schönberg (2010); von Wachter and Bender (2006); von Wachter *et al.* (2009)), mobility may very

[1] Among others, see for the US: Topel and Ward (1992); Neal (1999); Moscarini and Thomsson (2007); Kambourov and Manovskii (2008, 2009a); for France: Lalé (2012); for Germany: Fitzenberger and Kunze (2005), von Wachter and Bender (2006); von Wachter *et al.* (2009); Gathmann and Schönberg (2010); for Denmark: Groes *et al.* (2015); and for Germany and the UK: Longhi and Brynin (2010).

well be associated with career progression or job shopping in labor markets with frictions (Topel and Ward (1992)), thus resulting in wage gains after mobility (Groes *et al.* (2015); Fitzenberger and Spitz (2004); Fitzenberger and Kunze (2005)). Furthermore, mobility across firms and occupations may be an important adjustment mechanism in a dynamic labor market. For instance, the tasked-based approach introduced by Autor *et al.* (2003) argues that there is a decline in the demand for routine intensive occupations, to which workers may adjust through occupational mobility (Cortes (2016); Gathmann and Schönberg (2010)). Most of the literature referred to so far is restricted to an analysis of either job mobility or occupational mobility.[2] Based on high-quality administrative data, our analysis allows to distinguish the wage effects of job mobility and occupational mobility. In Germany, vocational training in an apprenticeship involves a job in the training firm and training in a specific occupation. Our analysis estimates the wage effects of mobility right after graduation from an apprenticeship in Germany.

Graduates from apprenticeship constitute a large share of the German workforce, and the apprenticeship combines practical training at the training firm with part-time school-based training, thus involving both general and occupation-specific skills.[3] Graduates may continue to work as a regular employee in their training firm, possibly in their training occupation or in another occupation. At graduation, there is no employment protection in the training firm. Given the combination of firm-based and school-based training the skills acquired during an apprenticeship are often thought to be largely transferable across jobs, thus allowing for worker mobility after graduation from apprenticeship (Euwals and Winkelmann (2002, 2004); Clark and Fahr (2002)). Indeed, retention rates are only about 60–75% of all graduates (Bougheas and Georgellis (2004); Euwals and Winkelmann (2004); von Wachter and Bender (2006)). The high mobility after graduation is a particularly interesting case to analyze. On the one hand, a change across firms involves the loss of the training investment for the training firm (Wolter and Ryan (2011)) and a change of occupation (firm) may imply a loss of the occupation- (firm-) specific human capital acquired through apprenticeship training (Kambourov and Manovskii (2008); Gathmann and Schönberg (2010)). On the other hand, firms may use the apprenticeship as a screening device for young workers, and they may only retain those apprentices after graduation who perform well (Euwals and Winkelmann (2002); Werwatz (2002); von Wachter and Bender (2006)). Graduates from apprenticeship may search for better job offers as a form of career progression (Topel and Ward (1992); von Wachter and Bender (2006); Fitzenberger and Spitz (2004)), and non-training firms may make attractive job offers to well trained graduates from apprenticeship, i.e. there is an incentive for poaching (Wolter and Ryan (2011)). A better match for the employee may also involve working in a different occupation within the training firm, an issue which has

[2] Studies which investigate mobility across firms and occupations include Neal (1999); Kambourov and Manovskii (2008); Longhi and Brynin (2010), or Mueller and Schweri (2015).

[3] For a detailed description of the German dual system of vocational training see e.g. Hoeckel and Schwartz (2010). A graduate from apprenticeship obtains a certified degree in one out of 350 training occupations. In 2009 about 60% of German youths aged between 16 and 24 years entered vocational training (Gericke *et al.* (2011)).

received little attention in the literature so far.

Several studies analyze the individual labor market effects of mobility after apprenticeship – mainly for Germany and Switzerland. However, the existing studies typically do not distinguish between a pure firm switch without occupation switch and a simultaneous switch of firm and occupation (a complex switch according to Neal, 1999), and occupational mobility within the training firm is typically ignored. von Wachter and Bender (2006) estimate a large immediate negative causal wage effect of a switch of firm after graduation. However, the negative effect vanishes five years afterwards. The study emphasizes that OLS estimates of the wage effects after five years are severely downward biased due to the negative selection of the firm switchers. In contrast, a negative wage effect of a firm switch is found by Bougheas and Georgellis (2004) for a six year period after training, and other studies find small positive wage effects of leaving the training firm (Euwals and Winkelmann (2004); Göggel and Zwick (2012)). For Switzerland, Mueller and Schweri (2009, 2015) find no wage differential between stayers and pure firm switchers one year after graduation from apprenticeship. Göggel and Zwick (2012) find a small negative immediate wage effect of a switch in occupation. Bougheas and Georgellis (2004) find a positive wage effect of a switch in occupation without switch of firm relative to stayers during the first six years after training. A simultaneous switch of occupation and firm is associated with wage losses both in Germany (Bougheas and Georgellis (2004)) and in Switzerland (Mueller and Schweri (2015)).

There exist some further studies considering mobility later during the career among prime-aged German workers holding an apprenticeship degree that provide further insights into the topic. Dustmann and Schönberg (2012) estimate the transferability of skills obtained through apprenticeship training for a sample of male workers. The survey data contains information provided by workers on how well they can apply skills obtained through apprenticeship training in their current job. Dustmann and Schönberg (2012) estimate that relative to stayers, pure firm switchers can apply 4.5% less of these skills in their current job. In their current job within-firm occupation switchers can use 8.6% less of their skills obtained through apprenticeship training, while across-firm occupation switchers can use up to 34% less of these skills. These results suggest that occupational mobility is associated with large losses in human capital, especially if a simultaneous firm change occurs. In contrast to this, Clark and Fahr (2002) find that only changes across 1-digit occupations entail wage losses while within 1-digit occupations the skills obtained through apprenticeship training are transferable. Regarding the wage effects of occupational mobility among prime-aged workers, other studies also draw a rather positive picture of occupation changes as they find average wage gains (Werwatz (2002); Fitzenberger and Spitz (2004); Fitzenberger and Kunze (2005)). Werwatz (2002) finds a negative wage effect of occupational mobility only for the small group of occupation switchers who state that in their current job they can only apply very little or none of the skills obtained through training. Similarly, Gathmann and Schönberg (2010) find that the wage loss implied by a switch in occupation increases with

the differences in task inputs between the source occupation and the target occupation.

Our study provides causal estimates of the wage effects of mobility across firms and occupations among graduates from apprenticeship in Germany. Our data consist of about 14.200 male graduates who completed apprenticeship training during the period of 1992–1997. We contribute both to the literature on the economic effects of occupational mobility as well as to the literature on labor mobility among young workers. Apprenticeship graduates are very likely selected into the different types of mobility based on unobservables, which may bias OLS estimates. We therefore employ an instrumental variables approach exploiting variation in regional labor market characteristics to estimate the causal short-term and long-term effects of mobility after apprenticeship on wages. We show that local labor market characteristics, such as the unemployment rate, labor market tightness and mobility behavior of the local workforce, are significantly correlated to the incidence of different types of mobility after graduation from apprenticeship.[4] Our paper also contributes to the discussion as to whether an apprenticeship, as part of the school-to-work transition, prepares well for a successful entry into the labor market. This aspect has been the subject of an intensive debate in several EU countries who are discussing reforms of vocational training in order to reduce the high level of youth unemployment (Bundesministerium für Bildung und Forschung (2012); The Economist (2013)).

We contribute to the literature on occupational mobility among young workers by carefully distinguishing between two different dimensions of mobility: mobility across firms and mobility across occupations. The literature on job mobility among young workers as well as the literature on occupational mobility typically does not distinguish these two dimensions and occupational mobility within the training firm is typically ignored.[5] Studies on occupational change often only consider across-firm occupation changes as valid, while within-firm occupation changes are perceived as "spurious" and stemming from coding errors (see e.g. Lalé (2012), and Longhi and Brynin (2010)). In our analysis, we use high-quality German administrative data. We can therefore distinguish four different mobility groups among apprenticeship graduates: stayers, pure firm switchers, within-firm occupation switchers and across-firm occupation switchers. Furthermore, we account for the heterogeneity of the estimated wage effects with regard to the wage position of the training occupation.

[4] Other studies on the individual labor market effects of mobility after apprenticeship in Germany deal with the endogeneity issue using a selection correction approach (Werwatz (2002); Bougheas and Georgellis (2004); Fitzenberger and Spitz (2004); and Mueller and Schweri (2015) for Switzerland) or they consider only displaced workers (Clark and Fahr (2002); Bougheas and Georgellis (2004); Göggel and Zwick (2012)). von Wachter and Bender (2006) use differences in firm-specific retention rates as exogenous variation. Neumark (2002) analyzes job mobility among young workers in the U.S. using local unemployment rates as instruments.

[5] An exception are Seibert and Kleinert (2009) who provide a descriptive analysis of mobility at the transition from apprenticeship training into the first job for Germany. Dustmann and Schönberg (2012) use mobility groups similar to our definition to estimate the extent of transferability of human capital across firms and/or occupations. Göggel and Zwick (2012) consider changes across employers and changes across occupations after apprenticeship, but it remains unclear whether these two groups are defined truly exclusively. Mueller and Schweri (2015) analyze occupational mobility after apprenticeship in Switzerland considering three well-defined groups similar to our definition of stayers, firm switchers and across-firm occupation switchers.

Our IV estimates imply that pure firm changes after graduation from apprenticeship lead to average wage losses of about 3.3–4.2% relative to stayers, although the long-term wage losses are reduced once we control for the training occupation. Regarding occupational mobility, the results differ strongly by whether there is a firm change. On average, job-and-occupation changes imply persistent wage losses of about 3.3–4.0% for a period of 7 years after entry into the first job relative to stayers. An occupation change within the firm results in persistent wage gains of about 12%. Within-firm occupation switchers are negatively selected and the switch allows the employee to move to an occupation which matches the employee's skills in a better way. Allowing for heterogeneous wage effects, we find that firm switchers and across-firm occupation switchers tends to lose less/benefit more with a lower relative wage position of the training occupation. In contrast, the wage gain of within-firm occupation switches increases in the relative wage of the training occupation. We further distinguish whether the employee moves to an occupation with a higher relative wage (upgrading) or to an occupation with a lower relative wage (downgrading). The results suggest that in the majority of cases an occupational switch involves a career progression. In contrast, for job switches the wage loss dominates – and the loss does not grow when there is an occupation switch at the same time.

The remainder of the paper is organized as follows: Section 2.2 discusses our identification strategy and the estimation approach. Section 2.3 describes the data used. Section 2.4 contains the empirical results. We present descriptive results and discuss the performance of the instrumental variables as well as the IV estimation results. Section 2.5 concludes. The Appendix 5.1 provides complementary empirical results (Tables and Figures starting with "A.") and further background information, such as a description of the data cleaning procedures and the matching of instrumental variables across spatial classifications.

2.2 Empirical Approach

2.2.1 Identification Strategy

We estimate the wage effects of mobility across firms and occupations up to seven years after graduation from apprenticeship. There are four treatments (mobility groups): Stayers, who do not switch neither their job nor their occupation, within-firm occupation switchers, job switchers within occupation, and job-and-occupation switchers (Table A.2). A comparison of average wages across the four mobility groups after controlling for observable characteristics would ignore potential selection effects in mobility based on unobservables. On the one hand, Acemoglu and Pischke (1998) and von Wachter and Bender (2006) find that job switchers are a negative selection. During apprenticeship training firms screen the ability of an apprentice and will only retain well-performing apprentices after graduation. By analogy, one would expect a negative selection of occupational switchers. A switch in occupation should be more rewarding for those graduates whose initial match with the training occupation was

Table 2.1: Main Instrumental Variables

Instrumental variable	**Level of variation**	**Data source**
unemployment rate	iabs-districts	FEA
unemployment rate $<$ 25 years	iabs-districts	IABS, FEA
vacancies/unemployed	empl. agency	FEA
mobility rates:		
job switch		
within-firm occ. switch	iabs-districts,	IABS
job-and-occ. switch	economic sector	
exit into unemployment $>$ 3 months		
share of low-skilled workers	empl. agency	FEA
share of high-skilled workers		

Notes: FEA: Federal Employment Agency, IABS: IAB Employment Sample regional file 1975–2004; Dummies for German federal states also included; Regarding the mobility rates, the set of instrumental variables further contains dummy variables (and interactions thereof with the mobility groups) controlling for small cell size and mobility rates of zero.

especially poor (Fitzenberger and Spitz (2004); Gathmann and Schönberg (2010)).

At the same time, to the extent that graduates choose to change their employer and/or occupation as a form of career advancement, mobility is more likely to occur if it leads to a wage increase relative to staying in the training firm and/or occupation (Topel and Ward (1992)). If this is the case, future wage prospects feed back into the mobility decision. This type of positive selection into mobility serves as another potential source of the endogeneity of mobility decisions. Previous work for Germany finds a positive selection of occupation switchers for older workers (Werwatz (2002); Fitzenberger and Kunze (2005)).

von Wachter and Bender (2006) point out that there is sorting into training occupations and training firms. On the one hand, one would expect that training firms with a low retention rate are attracting a worse pool of apprentices. On the other hand, able apprentices may choose a training firm with a low retention rate if the training is particularly useful for their career. von Wachter and Bender (2006) find that sorting into firms implies a negative selection of job switchers. In contrast, Dustmann and Schönberg (2012) find that including firm fixed effects leaves the regression estimates for the wage effect of mobility among graduates unchanged. Thus, we only account for selection into training occupations by including 2-digit training occupation fixed effects in the wage regressions.

To identify the causal effect of mobility after apprenticeship on wages, we use variation in the local labor market situation in the year of graduation. Our instruments involve both push and pull factors, such as indicators of the tightness of the local labor market and group specific mobility rates.[6] We argue that our instruments provide an exogenous variation in

[6]There are a number of studies which use similar instruments for mobility, see among others Neumark (2002); Mueller and Schweri (2015); Werwatz (2002) and von Wachter and Bender (2006).

mobility conditional on the sorting of apprentices by 2-digit training occupations, which we account for by including occupation fixed effects.

Our analysis uses data on the graduation cohorts 1992–1997 in West Germany. By the end of 1992 the reunification boom had come to a halt and the West German economy dropped into a deep recession which was accompanied by a worsening of labor market conditions and an increase in the unemployment rate. The recession was followed by a slow recovery until the late 1990's.[7] Thus, in addition to the regional variation, the indicators of the local labor market conditions used as instruments involve sizeable variation over time.

Table 2.1 summarizes the set of instrumental variables used. We use the aggregate local unemployment rate and the ratio of vacancies per registered unemployed to account for the business cycle in general. In addition, the unemployment rate for those below age 25 accounts specifically for the labor market changes for apprentices who are displaced by their training firm. We also include the shares of high-skilled and low-skilled workers to capture the educational background of the local workforce. The set of instruments also includes dummies for the German federal states, which differ in aggregate labor market conditions. Finally, as proxies for further local labor market characteristics that may affect mobility, we use regional mobility rates and exit rates into unemployment for male workers aged 25–35, where we exclude our apprenticeship graduates from the calculation.[8] Similar to von Wachter and Bender (2006), we use the mobility rates of other young workers as a proxy for local labor market characteristics that may affect the mobility of graduates from apprenticeship.[9]

The instrumental variables are matched to the sample of graduates from apprenticeship via the administrative district of the training firm and the year of graduation.[10] The way local labor market conditions affect mobility rates may differ across Germany, depending upon the labor market conditions in adjacent administrative districts and mobility patterns between different districts. Therefore, we allow the first stage regressions for the mobility dummies to differ by 26 West German regions.

To justify our identification strategy, our instruments must have a significant impact on mobility, and we need to discuss the necessary conditional exogeneity assumption. Pooled OLS estimations at the national level reveal a statistical significance of the instruments on the mobility dummies, see section 2.4.3 for details. For the time period under investigation, the exogeneity of the instruments for wages in West Germany (conditional on time effects accounting for the aggregate business cycle) is plausible because wages are basically de-

[7] For a detailed account, see (Sachverständigenrat, 1993, p. 3), (Sachverständigenrat, 1996, pp. 1 and 22), and (Sachverständigenrat, 1998, pp. 84–87).

[8] The exit rates into unemployment, where the unemployment spell lasts at least 92 days, are calculated only for workers who were full-time employees at the end of the previous year. Observations in years with at least one apprenticeship training episode are excluded.

[9] The set of instrumental variables further contains dummy variables for a small cell size. Year-administrative district-economic sector cells are small ($n < 10$ persons) for about 7.4% of all graduates. Furthermore, the distributions of mobility rates show spikes at zero (these results are available upon request), for which we also include dummy variables.

[10] For variables measured at the level of employment agencies, we constructed a key that allows us to match employment agency districts to administrative districts (for details see Appendix 5.1.1).

termined by collective wage bargaining between unions and employer associations at the industry level, and coverage by industry-level wage agreements varies between 70% and 62% of employment (Schnabel (2005)). Consistent with our line of argument, Mertens (2002) finds that in West Germany wages are rigid at the level of federal states, and that regional labor demand shocks have no significant effect on wages.

2.2.2 Estimation

We estimate the following pooled wage regressions separately for the time period 0–2 years (short term) and the time period 3–7 years (long term) in employment after graduation from apprenticeship:

$$\begin{aligned}\log(wage_{it}) =& \alpha + \beta_1 \cdot job\_sw_i + \beta_2 \cdot occ\_sw_i + \beta_3 \cdot occ\_job\_sw_i + \gamma \cdot X_i \\ &+ \sum_j \delta_j \cdot occup_{j,i} + \varepsilon \cdot yograd_i + \zeta \cdot yoempl_{it} + \eta \cdot year_{it} + u_{it}\end{aligned}$$

with the dummy variables job_sw_i, occ_sw_i, $occ\_job\_sw_i$ representing the three mobility dummies. In addition, we control for the following set of covariates (X_i): age at the beginning of the first job, diploma from upper track secondary schools (Abitur), non-German citizenship, and citizenship missing. All specifications include a set of dummies for year of graduation ($yograd_i$). We also add a dummy for each 2-digit training occupation j ($occup_{j,i}$) to control for selection into training occupations. Furthermore, all regressions control for the year since start of employment after graduation ($yoempl_{it}, t = 0, ..., 7$) and the calendar year ($year_{it}$). Standard errors are clustered at the person level.

To increase efficiency of the estimator, our instrumental variables (IV) approach takes account of the binary nature of the endogenous variables by estimating a Probit model in the first stage and by adopting GMM estimation in the second stage (Angrist (2001); (Wooldridge, 2010, chapter 21)). Specifically, we adapt Wooldridge's Procedure 21.1 as follows:

1. Estimate a Probit model separately for 26 regions for each mobility dummy controlling for the exogenous covariates X_i and the local labor market characteristics IV_i and calculate the predicted probabilities $\hat{P}_{i,mobtype}$:

$$\hat{P}_{i,mobtype} = \alpha + \gamma \cdot X_i + \sum_j \delta_j \cdot occup_{j,i} + \lambda \cdot IV_i + \varepsilon \cdot yograd_i + u_{it}$$

2. Estimate optimal cluster-robust GMM[11] using the three predicted probabilities $\hat{P}_{i,mobtype}$ from step 1 as excluded instruments for the endogenous mobility dummies.

This two-step procedure allows to use the usual GMM standard errors and test statistics and

[11] We estimate optimal cluster-robust GMM using Stata command *ivregress* with clustered standard errors.

it is robust against a misspecification in the Probit models (Wooldridge, 2010, chapter 21).

In a second set of results, we allow the mobility effects to differ by the relative wage position of the training occupation. To obtain the relative wage position, we regress log-wages on age, age^2, a full set of year dummies, and a full set of occupation dummies (without intercept) for full-time working males below age 30:

$$\log(wage_i) = \sum_j \beta_j \cdot occup_{j,i} + \alpha_1 \cdot age + \alpha_2 \cdot age^2 + \eta \cdot year_{it} + u_i$$

where β_j is the estimated relative wage position for occupation j. We define $tw(occup)_i = \sum_j \beta_j \cdot occup_{j,i}$ as the relative wage position of the training occupation of individual i, and we calculate the average relative wage position within each mobility group, denoted by $\overline{tw}_{mobtype}$. The wage regression now includes both the three mobility dummies and three interaction terms with the mobility dummy for mobtype times $(tw(occup)_i - \overline{tw}_{mobtype})$. Adapting (Wooldridge, 2010, Procedure 21.2), the second-step GMM estimation now uses both the three predicted probabilities $\hat{P}_{i,mobtype}$ and the three interaction terms $\hat{P}_{i,mobtype} \cdot (tw(occup)_i - \overline{tw}_{mobtype})$ as instruments. In addition, the set of instruments includes a third order polynomial of the relative wage position. The normalization of the relative wage position allows us to use the coefficient of the mobility dummy as the estimate of the average wage effect of mobility among the corresponding mobility group (ATT: average effect of treatment for the treated).

Based on the GMM estimates of the model with interaction effects, we calculate the estimated heterogeneous mobility effects at different deciles (q_j, with $j = 1,...,9$) of the relative wage of the training occupation as:

$$ATT_{q_j,mobtype} = coef_{mobtype} + (tw_{q_j,mobtype} - \overline{tw}_{mobtype}) \cdot coef_{(tw(occup)_i - \overline{tw}_{mobtype}) \cdot mobtype}$$

where $coef_{mobtype}$ is the coefficient of the mobility dummy and $coef_{(tw(occup)_i - \overline{tw}_{mobtype}) \cdot mobtype}$ is the coefficient of the interaction effect. We also calculate the treatment effects at different deciles of the entire sample.

2.3 Data

Our analysis is based on the IAB Employment Sample (IABS) regional file 1975–2004, a 2% random sample of all employees paying social security taxes (see Drews (2008)). The basic data involves employment spells and spells of unemployment benefit receipt. We restrict our sample to full-time working men in West Germany who completed their vocational training sometime during the period of 1992–1997 (Berlin is excluded). For employment spells, we observe daily wages, indicators of full-time and part-time work, the three-digit occupation code (about 130 occupations), and the industry. The dataset records a switch of establishment, but we do not know if two employees work in the same establishment. This

prevents us from estimating establishment fixed effects.

An ongoing apprenticeship is recorded as a regular employment spell with the status information apprentice. To identify the completion of the first apprenticeship training, we use the information about when there is change in the reported education to vocational training degree. Because of potential misclassification problems, we implement a series of data cleaning procedures and sampling conditions. A further complication stems from the fact that there can be a time lag between completion of the vocational training degree and the fact being recorded in the education variable in the IABS. Appendix 5.1.2 provides an overview of the data cleaning procedures and detailed further data preparation steps.

We determine mobility after apprenticeship based on changes in the occupational code (occupation switch) and changes in the establishment ID (job switch) between the employment spell recording the apprenticeship and the first job spell after graduation. Figure A.2 illustrates the timing of spells in a case with an employment interruption between apprenticeship and first job after graduation.

There is a lot of concern in the literature about measurement error in occupational codes when using survey data which is self-reported by the employee, see e.g. Neal (1999) for the US. In fact, Longhi and Brynin (2010) argue that occupational switches within firms are not well measured in household panel data of the SOEP for Germany and the BHPS for the UK. Our administrative data involve occupational codes reported by the employer, for which measurement error is likely to be very small (similar data are used by Fitzenberger and Kunze (2005), and Gathmann and Schönberg (2010)). It is likely that employers report precisely the occupation of the first regular job of an employee after graduation from apprenticeship. In fact, our data show a sizeable number of occupational switches within firms, which we can analyze in contrast to Longhi and Brynin (2010) .

We construct an unbalanced wage panel for full-time working males with a yearly frequency (Table A.3). Starting with the wage in the year of the first employment spell after graduation, we record the wage up to seven years after the year of the first employment spell. Wages are averaged across all employment spells observed in one year. Since the IABS data only contains information on daily wages, we only take full-time employment spells into account. In case of parallel employment spells, we only use the spell with the highest recorded wage. We drop records with zero wages and jobs where employees work at home (*Heimarbeit*, typically part-time). Wages are deflated by the consumer price index (2005=100) and measured in Euros.[12] We impute top-coded wages based on a Tobit model, for which we only know that the wage exceeds the social security contribution.

[12]The consumer price index is obtained from (Statistisches Bundesamt, 2010, p. 214).

Table 2.2: Summary Statistics for Four Groups of Apprenticeship Graduates

Variable	All graduates	Mobility type: stayers	job switchers	within-firm occ. switch	job-and-occ. switch
Total	14234	8316	2225	1198	2495
Share	1	0.58	0.16	0.08	0.18
Year of graduation					
1992	2362	0.61	0.15	0.08	0.16
1993	2483	0.60	0.16	0.08	0.16
1994	2495	0.56	0.16	0.08	0.19
1995	2342	0.60	0.14	0.10	0.16
1996	2237	0.56	0.17	0.09	0.19
1997	2315	0.57	0.16	0.08	0.19
High school diploma	0.10	0.10	0.12	0.08	0.07
Foreign citizenship	0.10	0.08	0.10	0.12	0.16
Citizenship missing	0.02	0.02	0.02	0.02	0.03
Apprenticeship duration	1076	1071	1072	1096	1083
Distance between graduation and first job (days)	49	6	107	6	160
Age at beginning of first job	20.83	20.72	21.08	20.77	21.01

2.4 Empirical Results

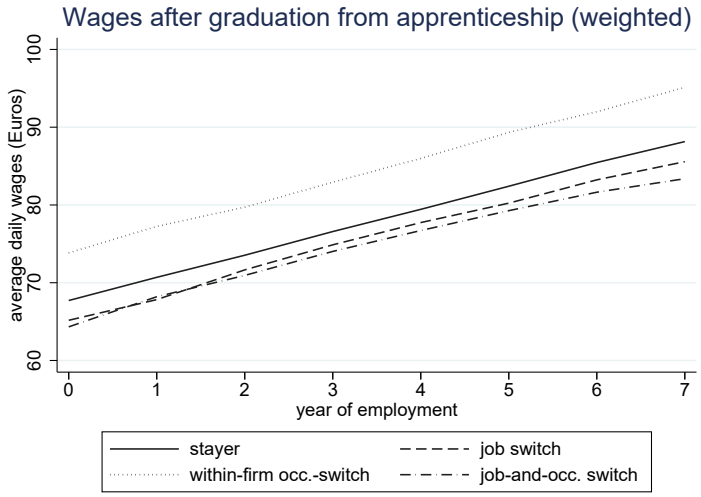
2.4.1 Descriptive Results

Table 2.2 shows descriptive statistics for the four mobility groups. Our sample consists of about 14.200 male apprenticeship graduates. While the four mobility groups differ in size, the sample shares do not vary a lot over the graduation years 1992 to 1997.[13] The stayers, i.e. those who stay with their training firm and their training occupation, form the largest mobility group. They will also serve as the comparison group in all further econometric analysis. Table 2.2 shows that, in comparison to stayers and job switchers, occupation switchers less often hold an upper secondary school degree and more often are of foreign citizenship. The average apprenticeship duration as well as the average age at the beginning of the first job after graduation are fairly similar across the four mobility groups. However, regarding the time it takes to start the first job, we observe strong differences between the four mobility groups. Stayers and within-firm occupation switchers quickly start their first job after graduation. In contrast, to start the first job after apprenticeship, it takes about 15 weeks for job switchers and 23 weeks for job-and-occupation switchers.

Figure 2.1, p. 18 displays the descriptive wage profiles for the four mobility groups weighted by the individual length of employment spells. All mobility groups show average wages that increase almost linearly with years of employment. However, wage levels differ across mobility groups. Within-firm occupation switchers earn higher wages than stayers.

[13] The overall share of graduates leaving the training firm in our sample is similar to that reported by von Wachter and Bender (2006) for the German apprenticeship graduation cohorts 1992–1994. The shares of mobility groups in our sample are also roughly consistent with the ones reported for mobility among German apprenticeship graduates in Seibert and Kleinert (2009).

Figure 2.1: Wages after Graduation from Apprenticeship



Notes: Observations weighted by length of employment spell.

The two groups of apprenticeship graduates who leave their training firm, job switchers and across-firm occupation switchers do worse than the stayers.

2.4.2 OLS Results

Table 2.3 shows the estimated wage effects of mobility obtained by a Pooled OLS wage regressions controlling for a set of socio-economic covariates. The results reported in columns (1) and (3) imply that on average within-firm occupation switchers earn about 7.5% higher wages than stayers in the short run (up to two years after entry into first job), and about 6.9% higher wages in the long run (years three to seven after entry into first job). In contrast, firm switchers do worse than stayers in terms of wages. Relative to stayers, wage losses for job switchers amount to about 3.5% in the short run and about 3.8% in the long run. Relative wage losses for job-and-occupation switchers are slightly more pronounced with losses of about 4% in the short run and about 4.9% in the long run. A comparison of short-run and long-run results suggests that wage differences are persistent and for both job switchers and job-and-occupation switchers no catching up takes place over a seven-year horizon after entry into the first job. However, as the results in Table 2.3 show, within each time window on average wages tend to increase over years of employment.

In addition, the specifications in columns (2) and (4) of Table 2.3 control for the 2-digit training occupation to account for possible sorting of apprentices into training occupation. The wage gains of within-firm occupation switches are stronger, both in the short and long run, compared to the results without controlling for the 2-digit training occupations. This suggests a negative selection regarding the training occupations of within-firm occupation switchers. The relative wage losses of job switchers and job-and-occupation switchers are

Table 2.3: Pooled OLS Estimates without Heterogeneous Treatment Effects

Dependant variable: log(wage)	Short term (0–2)		Long term (3–7)	
	(1)	(2)	(3)	(4)
Job switch	-0.0346***	-0.0251***	-0.0378***	-0.0222***
	[0.0056]	[0.0051]	[0.0066]	[0.0063]
Within-firm occ. switch	0.0753***	0.0841***	0.0690***	0.0734***
	[0.0077]	[0.0070]	[0.0087]	[0.0083]
Job-and-occ. switch	-0.0404***	-0.0353***	-0.0492***	-0.0395***
	[0.0061]	[0.0059]	[0.0069]	[0.0068]
Age at job entrance	0.0105***	0.0109***	0.0124***	0.00799***
	[0.0014]	[0.0013]	[0.0016]	[0.0016]
High school diploma	0.0388***	0.0489***	0.127***	0.0973***
	[0.0080]	[0.0083]	[0.0099]	[0.0105]
Foreigner	0.0244***	0.0106*	0.0270***	0.0175**
	[0.0065]	[0.0059]	[0.0075]	[0.0072]
Foreigner missing	-0.111***	-0.0942***	-0.124***	-0.0942***
	[0.0137]	[0.0128]	[0.0166]	[0.0165]
Year of employment 1	0.103***	0.0983***		
	[0.0067]	[0.0064]		
Year of employment 2	0.192***	0.180***		
	[0.0126]	[0.0121]		
Year of employment 4			0.0588***	0.0517***
			[0.0068]	[0.0066]
Year of employment 5			0.114***	0.100***
			[0.0134]	[0.0129]
Year of employment 6			0.165***	0.144***
			[0.0199]	[0.0191]
Year of employment 7			0.214***	0.186***
			[0.0265]	[0.0254]
Constant	4.182***	3.932***	4.274***	3.991***
	[0.0070]	[0.0289]	[0.0078]	[0.0322]
Fixed effects				
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
N	14225	14225	13378	13378
R-sq	0.060	0.192	0.067	0.134

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at person-level; Observations weighted by length of employment spell.

less pronounced after controlling for the 2-digit training occupation. This suggests that also these two mobility groups are negatively selected with respect to their training occupations. These results are similar to the findings of von Wachter and Bender (2006) regarding the negative selection of firm switchers regarding the training firms.

2.4.3 First Stage of IV Estimation

We exploit exogenous variation in local labor market conditions to instrument the different potentially endogenous mobility dummies. Our identification strategy is based on the assumption that the local labor market situation in the year of graduation is significantly correlated with graduates' propensity to leave the training firm and/or to switch occupation. From the first-step (stage zero) Probit regressions of the mobility decisions on the exogenous covariates and the local labor market conditions described in subsection 2.2.2, we obtain predicted probabilities $\hat{P}$ that then serve as the excluded instruments in the GMM estimation approach. When checking the validity of the above-mentioned assumption, we thus have to consider both the statistical relationship between the local labor market conditions (our original instruments) and the mobility decisions as well as the relationship between the predicted probabilities (our "constructed" instruments) and the mobility decisions.

As explained in subsection 2.2.2, in the first step of the IV procedures we also allow for heterogeneity regarding the influence of local labor market conditions on mobility decisions by estimating separate Probit regressions for 26 West German regions. We thus exploit the fact that the broader economic environment of the larger regions may mediate the way in which local labor market conditions (at the administrative district level) influence graduates' mobility decisions.

To summarize the relationship between the local labor market conditions and the mobility decisions, we run an OLS estimation at the national level for each of the three mobility groups. More specifically, we regress the predicted probabilities $\hat{P}$ obtained from the respective 26 initial Probit regressions on the set of exogenous covariates X_i and the local labor market conditions while pooling observations from all 26 regions:

$$\hat{P}_{i,mobtype} = \alpha + \gamma \cdot X_i + \lambda \cdot IV_i + \delta \cdot occup_i + \varepsilon \cdot yograd_i + u_{it}$$

The estimation results displayed in Table 2.4 show a statistically significant correlation between the local labor market conditions and the three different mobility dummies. When testing for joint statistical significance of the local labor market conditions, we obtain large F-statistics with values above 25. Patterns of individual significance and the signs of coefficients of local labor market conditions vary across the three regressions, thus showing that the different kinds of mobility decisions are affected in a different way by the local labor market conditions. The predicted probability of job switches within occupation appears to be driven by push factors. Whenever and wherever the local labor market conditions are worsening

Table 2.4: OLS Regression of Predicted Probabilities of Mobility on the Local Labor Market Conditions at the National Level (Pooling 26 Regions)

Dependent variable: Predicted probability of	Job switch (1)	Within-firm occ. switch (2)	Job-and-occ. switch (3)
Unemployment rate	0.0346***	-0.00345	0.0278***
	[0.0070]	[0.0065]	[0.0070]
Unemployment rate^2	-0.00371***	0.000582	-0.00198***
	[0.0007]	[0.0006]	[0.0007]
Unemployment rate^3	0.000114***	-0.0000101	0.0000663***
	[0.0000]	[0.0000]	[0.0000]
Unemployment rate < 25 years	0.00565***	-0.00387***	-0.00379***
	[0.0008]	[0.0008]	[0.0008]
Labor market tightness	-0.00180***	0.00118***	0.00130***
	[0.0004]	[0.0004]	[0.0005]
Labor market tightness^2	0.0000492***	-0.0000300***	0.00000746
	[0.0000]	[0.0000]	[0.0000]
Labor market tightness^3	-0.000000274***	0.000000170***	-0.000000136**
	[0.0000]	[0.0000]	[0.0000]
Share low qualified	-0.000840**	-0.000159	-0.0000146
	[0.0004]	[0.0004]	[0.0004]
Share highly qualified	0.00295***	0.000832**	0.00431***
	[0.0004]	[0.0004]	[0.0004]
Mobility shares			
Unemployment	-0.000214	-0.00374***	0.000407
	[0.0005]	[0.0005]	[0.0005]
Job switch	0.00236***	-0.00163***	0.00268***
	[0.0002]	[0.0002]	[0.0002]
Within-firm occ. switch	-0.00277***	0.00324***	0.00188***
	[0.0005]	[0.0005]	[0.0005]
Job-and-occ. switch	0.00190***	-0.00347***	0.0000289
	[0.0003]	[0.0003]	[0.0003]
Further instrumental variables			
Interaction effects indicating small cells for mobility shares	Yes	Yes	Yes
Interaction effects indicating mobility share zero	Yes	Yes	Yes
Fixed effects			
Year of graduation	Yes	Yes	Yes
2-Digit training occupation	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
N	14225	14225	14225
Adj. R-sq	0.280	0.212	0.295
F-test excl. IVs	25.78	42.01	32.36

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant; Year and year of employment dummies are not required since only one observation per apprenticeship graduate is included.

(increasing unemployment rates, lower labor market tightness), the predicted probability of firm change increases.[14] The opposite seems to hold for within-firm occupation switches. Here, an improving local labor market situation is correlated with a higher propensity to change occupation within the training firm. For job-and-occupation switches the picture is mixed. The predicted probability of job-and-occupation switches increases with higher overall unemployment, but decreases with higher youth unemployment (< 25 years), and it increases as the ratio of vacancies to unemployed improves. Thus, in the case of job-and-occupation switches both push and pull factors are significant.

As a proxy for further unobserved local labor market conditions that affect mobility, we have also included transition rates that vary at the local as well as the industry level for male workers aged 25–35. Thus, similar to von Wachter and Bender (2006) we use the mobility behavior of other young workers in the local labor market as a proxy for the individual graduate's propensity to change the firm and/or occupation. As Table 2.4 shows a certain higher overall mobility rate of young workers is always significantly positively correlated with the predicted probability of the respective mobility decision for apprenticeship graduates. Very clearly, within-firm occupation switches are less likely to occur in an environment with a higher exit rate into unemployment, with more job switches, or with more job-and-occupation switches. Regarding the determinants of job switches and job-and-occupation switches, the picture is somewhat mixed. Also, Table 2.4 implies that each type of mobility is more likely to occur if the local workforce involves a higher share of highly qualified employees and a lower share of employees with low qualifications.

Considering the statistical relationship between the predicted probabilities (our "constructed" instruments) and the mobility decisions, we find strong regional differences (Figures A.4-A.6). We exploit this variation in the instrumental variables approach and find highly statistically significant F-statistics for the excluded instruments (the "constructed" instruments) in the first stage of the GMM estimator (Table 2.5).

2.4.4 IV Estimates Without Heterogeneous Treatment Effects

We cannot assume random assignment into the four mobility groups for our sample of apprenticeship graduates conditional on the control variables considered in the OLS regressions. There is very likely selection into mobility, and from a theoretical perspective, both negative as well as positive selection effects could arise. Since an across-group comparison of average wage levels is likely to results in a biased estimate of the wage effects of mobility, we continue our analysis with estimating the causal effects of mobility after apprenticeship using an instrumental variables approach.

Table 2.6 displays the estimation results of the IV procedure (GMM, Wooldridge Procedure

[14] Mertens and Haas (2006) find a similar average relationship between regional unemployment rates and job mobility of workers for the period 1984–1999 in Germany. Furthermore, the workers were explicitly asked whether the job change was voluntary or involuntary. The authors find that rising local unemployment rates are related to higher involuntary job mobility and lower voluntary job mobility.

Table 2.5: Key Performance Measures for First Stages of IV Estimates

F-Test excl. IVs	Short term (0–2)		Long term (3–7)	
	(1)	(2)	(3)	(4)
A. Without Heterogeneous Treatment Effects:				
Job switch	265.6	492.1	250.9	467.3
Within-firm occ.-switch	221.6	402.9	226.5	411.3
Job-and-occ. switch	227.3	408.0	222.8	388.8
B. With Heterogeneous Treatment Effects:				
Job switch	158.3	271.5	140.7	250.7
Within-firm occ. switch	122.6	225.2	122.7	227.2
Job-and-occ. switch	125.6	215.2	120.4	205.3
$(tw(occup)_i - \overline{tw}_{job\_sw}) \cdot job\_sw_i$	239.9	222.6	226.1	236.2
$(tw(occup)_i - \overline{tw}_{occ\_sw}) \cdot occ\_sw_i$	60.3	89.9	65.2	89.9
$(tw(occup)_i - \overline{tw}_{occ\_job\_sw}) \cdot occ\_job\_sw_i$	180.0	142.9	165.9	134.7
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes

Table 2.6: Coefficient Estimates for IV Procedure without Heterogeneous Treatment Effects

Dependant variable: log(wage)	Short term (0–2)		Long term (3–7)	
	(1)	(2)	(3)	(4)
Job switch	-0.109***	-0.0429***	-0.123***	-0.0373**
	[0.0231]	[0.0155]	[0.0271]	[0.0184]
Within-firm occ. switch	0.232***	0.143***	0.238***	0.124***
	[0.0233]	[0.0179]	[0.0285]	[0.0219]
Job-and-occ. switch	-0.0241	-0.0333*	-0.0327	-0.0305
	[0.0257]	[0.0184]	[0.0281]	[0.0215]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes
N	14225	14225	13378	13378
Adj. R-sq	0.011	0.186	0.026	0.131

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at person-level; Observations weighted by length of employment spell; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant.

21.1) discussed in subsection 2.2.2. On average, wage losses due to job switches amount to about 4.3% (column (2)) in the short run and are largely persistent over time. This suggests that no catching up takes place relative to stayers, a result which differs from the results obtained by von Wachter and Bender (2006) for all job switches. The negative wage effect of a job switch is more pronounced than in the OLS regression (compare Table 2.3). This suggests a positive selection of job switchers into mobility.

The IV estimates also imply a causal wage effect of within-firm occupation switches that is much stronger than in the OLS regression. An occupation switch within the training firm results in an average wage gain of about 14.3% in short run relative to stayers (column (2)). These gains are largely persistent for a period of up to seven years after entry into the first job. A comparison of IV and OLS estimation results suggests a negative selection of within-firm occupation switchers.

Regarding the job-and-occupation switchers, the IV estimation results reveal a negative causal wage effect of leaving both the training firm and the training occupation. However, the effect is only statistically significant in the short run, amounting to an average wage loss of about 3.3% relative to stayers (column (2)). Since the long-run estimate is insignificant, some catching up relative to stayers may be possible in the long run (column (4)). The comparison to OLS results tends to imply a negative selection of job-and-occupation switchers.[15]

While the OLS estimation results suggest that all three mobility groups are negatively selected with respect to the 2-digit training occupation, comparison of IV specifications in Table 2.6 with and without 2-digit training occupation fixed effects shows a different pattern for job switchers. Here, job switchers are revealed to be positively selected into the training occupation. The IV results still indicate that within-firm occupation switchers are negatively selected with respect to the training occupation, while the results are somewhat inconclusive for job-and-occupation switchers.

2.4.5 Overidentification Test and Reducing the Number of Instruments

We use a large number of instruments when constructing the predicted probabilities $\hat{P}_{i,mobtype}$. This provides the opportunity to investigate the validity of the instruments by means of an overidentification test.[16] However, a standard overidentification test is not applicable for two reasons. First, we implement a GMM estimation approach which is formally based on the predicted probabilities as instruments. Thus, the GMM objective in the second stage can not be used for an overidentification test, simply because formally we have an exactly identified

[15] As a robustness check, we re-estimate the model shown in Table 2.6 with different clustered standard errors. We are grateful to a referee for this suggestion. Once we cluster the standard errors in the last step of the GMM estimation at the region - interacted with year-of-graduation level, which is the level at which a number of the instruments vary, instead of at the person level, standard errors do increase slightly. However, the significance level of the estimated coefficients only changes in two cases (Table A.4). Thus, we conclude that our results are basically robust to this change.

[16] We are grateful to one referee for suggesting to implement an overidentification test for our case.

case. Second, even though our estimates are second stage GMM estimates building on the weighting matrix estimated in the first stage, we argue that inference has to take account of clustering at the individual level. This is because the weighting matrix estimated in the first stage and used in the second stage does not account of clustering. Note further that our instruments are assumed to affect the endogenous treatment dummies through the nonlinear function yielding $\hat{P}_{i,mobtype}$.

As a simple approach to implement an overidentification test, we extend the heteroscedasticity-robust test of overidentifying restrictions for the two-stage least squares estimator suggested by (Wooldridge, 2010, p. 136) to our setting as follows. (i) We first run a panel regression of all instruments on the three $\hat{P}_{i,mobtype}$'s and on the exogenous regressors in the wage equation. Denote the residuals from this auxiliary regression as $\hat{r}_2$. (ii) Next, we regress the estimated residuals of the wage regression (these residuals are based on the GMM coefficient estimates and the actual treatment dummies plugged into the wage regression) on the residual variation of the instruments $\hat{r}_2$. (iii) We use the cluster robust Wald test statistic for the joint significance of all instruments in the regression under (ii). Because we estimate separate probit regressions by 26 regions to estimate $\hat{P}_{i,mobtype}$, our instruments are fully interacted with the regions. For this reasons, we implement the overidentification test (i)-(iii) separately by the 26 regions. Furthermore, we weight all regressions by the employment weight for each single wage observations. As a caveat, it should be noted that the auxiliary regressions involve a linear approximation of the possibly nonlinear relationship between the instruments and the error term in the wage regression.[17] Furthermore, we differ from (Wooldridge, 2010, chapter 6) by using all instruments in our auxiliary regressions in (i) and (ii), simply because $\hat{P}_{i,mobtype}$ is not a linear function of the instruments and therefore the matrix spanned by $\hat{r}_2$ has full rank.

The benchmark specification discussed in table 2.6 is based on 22 instruments. The overidentification test (see Table A.5, Panel 0) typically does not lead to a rejection at the 1% significance level for a majority of regions, but depending on the case considered there are between 5 and 13 rejections among 26 regions. The rejection rate is considerably higher than the significance level of 1%, and also the joint test for the national level involves a rejection. Thus, strictly speaking, our IV approach does not pass the overidentification test.

To address the problem in more detail, we now reduce sequentially the set of instruments from 22 to 7 instruments, as described in the notes of Table A.5. The 7 core instruments involve the general indicators of regional labor market conditions such as the unemployment rate, the unemployment rate below age 25, and labor market tightness at the district level (as well as powers of these variables). The 9 IV's also involve the skill shares among employees. And the 12 IV's involve information on the mobility share regarding unemployment longer than 3 months. For the 12 IV's, we have excluded the information on mobility shares regarding job switches and occupational switches whose effects we are estimating. Thus,

[17] Thus, a misspecification of the probit models for the treatment dummies may also cause a rejection of the overidentification test even though the instruments may still be strictly exogenous.

one might be most concerned about the validity of these group instruments. When we reduce the number of instruments, the number of rejections of the overidentification test falls dramatically. With 7 instruments there is no rejection any more for the long run with occupation dummies and there are only between one to three rejections for the other case (for all rejections the p-value lies around 0.005, except for one case with 0.0005). We take the model with 7 instruments as basically passing the overidentification test. One could make a similar argument for the model with 9 instruments. Regarding the number of rejections, the model with 12 IV's lies somewhere in between the model with 22 IV's and the model with 9 IV's.

Should we now use the model estimates based on 7 or 9 instruments as our benchmark model? A comparison of the estimated treatment effects in table 2.7 suggests that the variation of the set of instruments does not change the estimated treatment effects in a considerable way.[18] In particular, there is a striking qualitative similarity of the results (in light of the estimated standard errors) for the estimates with fixed effects for the 2-Digit training occupations). If the validity of the instruments were to be questioned, we would expect that the estimated treatment effects would change strongly. However, that is not the case. We rather think that the rejections suggest a slight misspecification of the probit model used to construct the predicted treatment probabilities $\hat{P}_{i,mobtype}$. Put differently, we do not have a problem of endogenous instruments but rather the nonlinearity of the relationship between the instruments and the treatment dummy variables may not be fully captured by our probit model. This problem may be aggravated by the fact that our mobility shares are noisy estimates and that we account for the fact that cell sizes are too small.

Because the IV approach suggested by (Wooldridge, 2010, p. 939), which we use in this paper, explicitly allows for a misspecification of the Probit model, we stick to our benchmark estimates with 22 instruments. Furthermore, the subsequent analysis will also be based on the set of 22 instruments.

2.4.6 IV Results With Heterogeneous Treatment Effects

The IV estimations presented so far estimate treatment effects that are homogeneous with respect to the relative wage position of the training occupation. However, differences in the relative wages of training occupations may reflect differences in the amount of occupation-specific capital typically obtained trough training as well differences in the occupation-specific ratio of labor supply and demand. In the following we will thus drop the assumption of homogeneous starting conditions within mobility groups by taking account of the relationship between the relative wage of the training occupation and the wage effects of mobility. The IV procedure (Wooldridge Procedure 21.2) discussed in Subsection 2.2.2 estimates the ATT, taking account of the effect heterogeneity by the relative wage level of

[18] The strongest difference involves the absolute reduction in the negative wage effect of a job switch in the models without fixed effects for the 2-Digit training occupations.

Table 2.7: Comparison of Coefficient Estimates for IV Procedure without Heterogeneous Treatment Effects for Various Sets of IVs

Dependant variable: log(wage)	Short term (0–2)		Long term (3–7)	
	(1)	(2)	(3)	(4)
A. 12 IVs (instead of 22 as in Table 2.6)				
Job switch	-0.0702**	-0.0303*	-0.0745**	-0.0212
	[0.0276]	[0.0176]	[0.0325]	[0.0206]
Within-firm occ. Switch	0.266***	0.150***	0.283***	0.114***
	[0.0297]	[0.0205]	[0.0351]	[0.0263]
Job-and-occ. Switch	0.0170	-0.0198	-0.0171	-0.0261
	[0.0302]	[0.0209]	[0.0338]	[0.0245]
B. 9 IVs (instead of 22 as in Table 2.6)				
Job switch	-0.0421	-0.0322*	-0.0579*	-0.0248
	[0.0296]	[0.0182]	[0.0347]	[0.0215]
Within-firm occ. Switch	0.265***	0.146***	0.261***	0.106***
	[0.0323]	[0.0227]	[0.0373]	[0.0281]
Job-and-occ. Switch	0.0393	-0.0215	0.00561	-0.0198
	[0.0320]	[0.0214]	[0.0362]	[0.0252]
C. 7 IVs (instead of 22 as in Table 2.6)				
Job switch	-0.0430	-0.0330*	-0.0727**	-0.0277
	[0.0311]	[0.0187]	[0.0369]	[0.0221]
Within-firm occ. Switch	0.229***	0.124***	0.233***	0.0892***
	[0.0334]	[0.0234]	[0.0390]	[0.0292]
Job-and-occ. Switch	0.0199	-0.0314	-0.0181	-0.0284
	[0.0335]	[0.0221]	[0.0382]	[0.0261]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at person-level; Observations weighted by length of employment spell; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant.

Table 2.8: Coefficient Estimates for IV Procedure with Heterogeneous Treatment Effects

Dependant variable: log(wage)	Short term (0–2)		Long term (3–7)	
	(1)	(2)	(3)	(4)
Job switch	-0.0317	-0.0354**	-0.0522**	-0.0270
	[0.0204]	[0.0150]	[0.0253]	[0.0179]
Within-firm occ. switch	0.203***	0.122***	0.211***	0.116***
	[0.0207]	[0.0161]	[0.0248]	[0.0185]
Job-and-occ. switch	-0.0106	-0.0399**	-0.0155	-0.0362*
	[0.0225]	[0.0173]	[0.0260]	[0.0206]
$(tw(occup)_i - \overline{tw}_{job\_sw}) \cdot job\_sw_i$	-0.100	0.00127	-0.160*	-0.126
	[0.0626]	[0.0642]	[0.0848]	[0.0856]
$(tw(occup)_i - \overline{tw}_{occ\_sw}) \cdot occ\_sw_i$	0.300	0.484***	0.513**	0.619***
	[0.1953]	[0.1827]	[0.2432]	[0.1965]
$(tw(occup)_i - \overline{tw}_{occ\_job\_sw}) \cdot occ\_job\_sw_i$	-0.342***	-0.199*	-0.291**	-0.190
	[0.1092]	[0.1063]	[0.1206]	[0.1193]
$tw(occup)_i$	0.989***	0.839***	0.893***	0.742***
	[0.0382]	[0.0514]	[0.0447]	[0.0581]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes
N	14221	14221	13374	13374
Adj. R-sq	0.172	0.234	0.123	0.156

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant.

the training occupation.

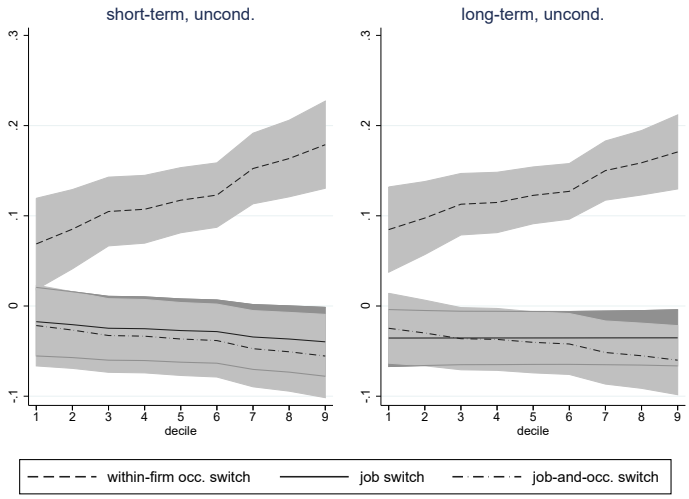
The main mobility effects shown in Table 2.8 are calculated as average effects among the corresponding mobility group. Regarding these average causal mobility effects the results do not change much relative to the IV results without heterogeneous treatment effects. For job switchers and within-firm occupation switchers the effects are a bit less pronounced than before. For job-and-occupation switchers the negative long-term wage effect now becomes statistically significant.

Regarding the relevance of the training occupation, Table 2.8 shows that on average the relative wage of the training occupation $tw(occup)_i$ is positively related to current wages both in the short and long run.[19] This means that apprenticeship graduates from training occupations with a higher relative wage also earn higher wages during the first seven years of their labor market careers.

Most importantly, the interaction effects between the relative wage distance and the mobility type reveal interesting results. Job-and-occupation switchers display negative

[19] The respective coefficients on $tw(occup)_i$ have to be interpreted as elasticities: On average a 1% higher wage in the training occupation is associated with a β% higher wage after graduation.

Figure 2.2: Average Treatment Effect on the Treated at Deciles of the Overall Distribution of Wages in the Training Occupation (Showing 95% Confidence Bands)



Notes: Calculations based on results from 3-step IV estimation controlling for 2-digit training occupations.

interaction effects. For job switchers, the interaction effect is close to zero in the short run, but becomes negative in the long run. Interestingly, the relationship is reversed for within-firm occupation switchers. Here, we find a positive interaction effect.

To illustrate the meaning of these findings, Figure 2.2 shows the ATT at deciles of the overall distribution of wages in the training occupation for each of the three treatment groups.[20] For job switchers and job-and-occupation switchers we find that those members of the mobility group who have been trained in a low-wage training occupation suffer relatively less from being mobile (relative to those having been trained in better-paid training occupations). We cannot rule out, that for the most ill-positioned graduates the respective mobility decision may even be neutral relative to stayers in terms of wages. Interestingly, the ATTs for job switchers and job-and-occupation switchers are not statistically different from each other. This suggests that additional to leaving the training firm a change of occupation does not have any further negative wage effects for the apprenticeship graduate. A change of occupation within the training firm is clearly beneficent for the apprenticeship graduates. Those apprenticeship graduates who, regarding their choice of training occupation, are already in a favorable initial position profit most from an occupational switch within the training firm. Even the initially most ill-positioned graduates profit from a within-firm occupation switch relative to stayers. [21]

Note that Figure 2.2 shows very similar patterns both for the short-term and the long-term

[20] A conditional version of Figure 2.2, where we compute the ATT at deciles of the group-specific distribution of wages in the training occupation, shows similar results (Figure A.7).

[21] Relative to the group of stayers, in the group of job-and-occupation switchers and even more so in the group of job switchers weakly ranked training occupations are more frequent. The group of within-firm occupation switchers is more dominant in the upper part of the ranking of training occupations (Figure A.8).

Table 2.9: Pooled OLS Estimates with Heterogeneous Treatment Effects

Dependant variable: log(wage)	Short term (0–2)		Long term (3–7)	
	(1)	(2)	(3)	(4)
Job switch	-0.00510	-0.0147***	-0.0129**	-0.0131**
	[0.0051]	[0.0050]	[0.0063]	[0.0064]
Within-firm occ. switch	0.0658***	0.0668***	0.0614***	0.0585***
	[0.0074]	[0.0072]	[0.0083]	[0.0083]
Job-and-occ. switch	-0.0279***	-0.0348***	-0.0370***	-0.0391***
	[0.0058]	[0.0058]	[0.0068]	[0.0068]
$(tw(occup)_i - \overline{tw}_{job\_sw}) \cdot job\_sw_i$	-0.103**	-0.0622	-0.121*	-0.124*
	[0.0470]	[0.0476]	[0.0692]	[0.0703]
$(tw(occup)_i - \overline{tw}_{occ\_sw}) \cdot occ\_sw_i$	0.211*	0.170	0.272**	0.278**
	[0.1168]	[0.1193]	[0.1370]	[0.1320]
$(tw(occup)_i - \overline{tw}_{occ\_job\_sw}) \cdot occ\_job\_sw_i$	-0.393***	-0.378***	-0.346***	-0.314***
	[0.0725]	[0.0730]	[0.0835]	[0.0846]
$tw(occup)_i$	1.030***	0.948***	0.941***	0.830***
	[0.0298]	[0.0427]	[0.0343]	[0.0480]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes
N	14221	14221	13374	13374
R-sq	0.200	0.240	0.148	0.161

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at person-level; Observations weighted by length of employment spell; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant.

results. Within-firm occupation switchers persistently perform better than stayers in terms of wages. For job switchers and job-and-occupation switchers the treatment effects appear to be largely persistent over time. However, one has to keep in mind that the average long-term effect of pure firm changes reported above turns insignificant once we control for selection into training occupations, so that for job switchers catching up relative to stayers could be possible.

A comparison of the IV estimation results relative to the corresponding OLS results in Table 2.9 shows similar selection patterns as discussed above in Section 2.4.4 for IV estimation without heterogeneous treatment effects. The estimation results indicate a positive selection of job switchers. Within-firm occupation switchers are negatively selected with respect to unobservables. This result holds in particular in light of the differences in the corresponding interaction effects between OLS and IV estimation with heterogeneous treatment effects. However, the IV results imply that there is no significant selection on unobservables for job-and-occupation switchers. A comparison of IV specifications in Table 2.8 with and without 2-digit training occupation fixed effects again suggests sorting into training occupations for all mobility groups.

2.4.7 Occupational Upgrading and Downgrading

Not only may the effects of occupational mobility depend on the initial occupational position of the apprenticeship graduates, but they may also be related to the direction of the occupational move. We explore this aspect of occupational mobility by distinguishing between upward and downward switches. Based on relative wages, we ordinarily rank all 130 occupations observed in the IABS from lowest paid (1) to highest paid (130). For each apprenticeship graduate we then compare the rank of his training occupation to the rank of his occupation in the first job after graduation and thereby determine whether they performed an upward or downward occupational switch. We find that in both mobility groups a significant proportion of occupational switches is directed towards higher ranked occupations. About 60% of within-firm occupation changes are upward. Surprisingly, even in the group of job-and-occupation switchers about 48% of all cases are associated with an upward move.

In light of these results, we estimate a modified version of the IV procedure without heterogeneous treatment effects that distinguishes between upward and downward occupation switches. As Table A.7 shows, the wage effects of occupational mobility are indeed heterogeneous with respect to the direction of the occupational move.[22] For within-firm occupation switchers we find that even those apprenticeship graduates who move towards a lower ranked occupation on average still realize significant relative wage gains of about 6.6% that largely persist over a seven year period after graduation.

Most importantly, we find that an occupation switch across firms does not necessarily cause a negative wage effect. Those job-and-occupation switchers who move towards a higher ranked occupation do not suffer wage losses on average. In the short run, they even realize significant average wage gains of about 6.7% relative to the stayers. In the long run, upward job-and-occupation switches appear to be at least wage neutral. These effects are strongest when we include fixed effects for the 2-digit training occupation and, thus, only compare job-and-occupation switchers moving away from the same initial 2-digit training occupation.

2.5 Conclusions

Distinguishing carefully between mobility across firms and across occupations, this study provides causal estimates of the wage effects of mobility among graduates from apprenticeship in Germany during the first seven years after starting the first regular job after graduation. Our analysis distinguishes between pure firm switchers, within-firm occupation switchers, and across-firm occupation switchers. Mobility across firms and occupations may be associated with a loss of human capital implying a wage loss or with finding a better job match implying

[22] The corresponding OLS estimation results can be found in Table A.6. Table A.8 provides results on the correlation between the local labor market conditions and upward/downward occupational mobility. Table A.9 shows the F-statistics for the excluded instruments in the first stage of the GMM estimation.

a wage gain. Due to the likely presence of selection based on unobservables, OLS estimates are likely to be biased and we employ an instrumental variables approach exploiting variation in regional labor market characteristics. We show that local labor market conditions, such as the unemployment rate, labor market tightness and mobility behavior of the local workforce, are significantly correlated with mobility after graduation from apprenticeship. Our analysis accounts for the heterogeneity of the estimated wage effects with regard to the wage position of the training occupation.

Our IV estimates imply that pure firm changes after graduation from apprenticeship lead to average wage losses of about 3.3–4.2% relative to stayers, although the long-term wage losses are reduced once we control for the training occupation. Job switchers are positively selected into mobility with respect to unobservable characteristics relative to stayers.

Regarding occupational mobility, the results differ strongly by whether there is a firm change. On average, job-and-occupation switches imply persistent wage losses of about 3.3–4.0% for a period of 7 years after entry into the first job relative to stayers. An occupation switch within the training firm results in persistent wage gains of about 12%. Our results indicate that Across-firm occupation switchers basically show no selection on unobservables, while within-firm occupation switchers are negatively selected. During the training period the employer can observe the apprentice's ability and then decide, whether the employee should switch to an occupation which matches the employee's skills in a better way. This occurs in particular when the initial match with the training occupation was poor.

Allowing for heterogeneous wage effects, we find that job switchers and across-firm occupation switchers tend to lose less/benefit more with a lower relative wage position of the training occupation. In contrast, the wage gain of within-firm occupation switchers increases in the relative wage of the training occupation. Furthermore, we find that the wage effects of occupational mobility differ by the direction of the move. Occupational upgrading across firms, which comprises 48% of all job-and-occupation switches, actually causes an average wage gain of 6.7%.

While our results indicate that pure firm changes after apprenticeship lead to wage losses, our conclusions regarding the wage effects of occupational mobility after apprenticeship are somewhat more positive. Occupational mobility within the training firm can be interpreted as a career progression involving persistent wage gains. The positive wage effects of occupation switches within the firm and occupational upgrading across firms suggest that for the majority of cases a change of occupation involves a career progression. In contrast, for job switches the loss of firm-specific human capital seems to dominate – and the loss does not grow when there is an occupation switch at the same time. At a more general level, our results suggest that the skills acquired through apprenticeship training in a specific occupation are sufficiently general to be useful when working in another occupation.

3 Occupational Mobility in the West German Labor Market

3.1 Introduction

Over the past decade there has been a growing awareness of the economic significance of worker mobility across the occupational dimension. It has been argued that the perception of increased economic turbulence in the US labor market could actually be reflecting the increase in occupational mobility which has been observed since the 1970s (Kambourov and Manovskii (2008)). From the point of view of an individual worker such an increase in occupational mobility could be economically harmful since the returns to occupational tenure are significant (e.g. Kambourov and Manovskii (2009b); Groes (2010)) and occupational switches should entail a loss of occupation-specific human capital (Gathmann and Schönberg (2010)). However, occupational mobility could also serve as a means for workers to improve the quality of their individual worker-job-match in a frictional labor market, and could then be interpreted as a form of career progression that results in wage gains (Fitzenberger and Spitz (2004); Fitzenberger and Kunze (2005); Longhi and Brynin (2010); Fitzenberger *et al.* (2015); Groes *et al.* (2015)).

At a broader level, occupational mobility is thought to be related to patterns of overall wage inequality (Kambourov and Manovskii (2009a); Cortes (2016)). It may allow workers to adjust to task-specific changes in labor demand induced by technological change (Cortes (2016)). This aspect of occupational mobility is especially important given that we are apparently witnessing the onset of a new period of accelerated technological change, which has been put into motion by the recent development of key technological capabilities that will most likely allow for the automation of non-routine tasks in the near future (e.g. Frey and Osborne (2013); Brynjolfsson and McAfee (2014)). Furthermore, occupational switches can have a polarizing tendency in the sense that highly-paid workers within an occupation tend to switch to even better paid occupations, while the reverse holds for the low earners within an occupation (Groes *et al.* (2015)).

Against this background, this paper contributes to the literature on worker mobility by providing a comprehensive overview of patterns of occupational mobility in the West German labor market for the period 1982–2008. In contrast to most related studies, it explicitly differentiates between across-firm and within-firm occupational mobility, and describes how both phenomena have evolved over time. The paper also assesses potential explanations for

the observed patterns of occupational mobility.

While interest in worker mobility across occupations is growing among economists, empirical evidence on the overall extent and the dynamics of occupational mobility in the labor markets of developed economies is still relatively scarce. Several papers analyze the extent of and trends in occupational mobility in the United States. Kambourov and Manovskii (2008) document a wide range of key findings on occupational mobility in the US labor market over the period 1986–1997. They estimate that the annual rate of 3-digit occupational mobility of male workers rose from 16% in the early 1970s to about 20% in the mid 1990s. Amongst other results, they find that patterns of occupational mobility vary by age and education. Their analysis suggests that, had the demographic structure of employment not changed since 1980, the increase in occupational mobility rates would have been even more pronounced. Additionally their analysis of net flows of workers across occupations suggests that the observed increase in occupational mobility could be the result of an increase in the variation of occupation-specific labor demand. The finding that occupational mobility increased from the 1970s to the mid 1990s is also corroborated by Moscarini and Thomsson (2007), who report that during the time period 1979–2006 monthly occupational mobility rates in the US were at about 3.5% of overall employment. However, they find that after the mid 1990s monthly occupational mobility rates decreased sharply. Parrado *et al.* (2007) report annual occupational mobility rates at the 1-digit level in the US that rose from 15–20% in the years 1969–1980 to 20–25% during the period 1981–1993 for male workers, and from 10–15% to 15–20% for female workers. The authors also consider sociodemographic determinants of occupational mobility and find that occupational mobility is lower for female than for male workers, and it is decreasing in age. In a more recent paper on France, Lalé (2012) finds an average occupational mobility rate of 7.4% at the 3-digit level with no overall trend over the observation period. However, after correcting for aspects of demographic change such as the aging of the French population, the author finds significantly higher occupational mobility rates that strongly increased over the period 1982–2009. Longhi and Brynin (2010) report that in Britain over the period 1991–2006 the average occupational mobility rate at the two-digit level amounts to 29.4%, but reduces to 8.6% if only occupational changes accompanied by a reported job change are considered. Furthermore, they show that the probability of an occupational switch across employers increases if workers are over-qualified for the current job. Occupational mobility across employers is associated with higher wage growth and improved job satisfaction relative to immobile workers. For Denmark, Groes *et al.* (2015) report that every year about 20% of workers change their occupation.

Empirical evidence on the overall patterns and the dynamics of occupational mobility in Germany is likewise scarce. Based on data on West Germany from the German Socioeconomic Panel Longhi and Brynin (2010) report an average occupational mobility rate of 11.8% at the 2-digit level over the period 1984–2006, which reduces to 3.2% if only occupational changes accompanied by a reported job change (which involves a change of

employer in about 70% of cases) are taken into account. The authors focus on the analysis of the individual-level determinants and labor market outcomes of occupational switches across employers. They show that the probability of an occupational switch across employers increases if workers are either over- or under-qualified for the current job. They find that occupational mobility across employers is associated with higher wage growth and improved job satisfaction relative to immobile workers as well as relative to job changes that do not involve an occupational switch. Isaoglu (2010b) also provides an analysis of the individual-level determinants of occupational switches in West Germany based on data from the German Socioeconomic Panel. According to the data-cleaning procedures described in a supplementary paper (Isaoglu (2010a)), the author considers a measure of overall occupational mobility that includes both occupational switches within and across employers as long as they are associated with a reported change in job status. Interestingly, Isaoglu (2010b) finds that the probability of an occupational switch is negatively related to having switched occupations in the previous year, as well as to increases in the local unemployment rate. Additionally, the author describes patterns in overall 4-digit occupational mobility over the period 1984–2004. Over the period of observation the overall annual 4-digit occupational mobility rate in West Germany averages about 5% when only employment-to-employment transitions are considered. If non-employment-to-employment transitions are considered as well, the average overall occupational mobility rate is higher (Isaoglu (2010a)). In line with the negative correlation between the unemployment rate and occupational switches, overall occupational mobility is pro-cyclical, but the author observes no trend in overall occupational mobility over the period of observation.

Bachmann and Burda (2007) use administrative data provided by the Research Data Centre of the German Federal Employment Agency at the Institute for Employment Research (IAB) to analyze sectoral transformation and labor market dynamics in West Germany over the period 1975–2001. While their analysis focuses on worker reallocation across industries, they also provide results on overall 3-digit occupational mobility rates, that comprise occupational switches both within and across establishments. The authors show that sectoral and occupational turbulence as measured by Lilien indices increased in the mid 1990s in West Germany. They document that over the period 1980–2000 the average annual overall 3-digit occupational mobility rate pertaining to employment-to-employment transitions is about 6% for male workers aged 16–29. Interestingly, Bachmann and Burda (2007) show that average overall occupational mobility rates are higher if they consider non-employment-to-employment transitions and even higher if they consider unemployment-to-employment transitions (11% and about 59% for young male workers, respectively). The authors find that gross overall 3-digit occupational mobility involving employment-to-employment transitions is pro-cyclical. The probability of occupational mobility conditional on leaving unemployment has increased over time, which the authors interpret as a sign that West German unemployed have grown occupationally more mobile over the period 1980–2000.

Also regarding the evolution of occupational mobility over time, Seibert (2007) documents patterns of occupational mobility for young workers in the specific context of labor market entry after graduation from an apprenticeship. Comparing the training occupation and the occupation in the first job based on IAB administrative data, he reports that overall 3-digit level occupational mobility rates for male graduates of the German apprenticeship system rose from about 18% in the 1970s to about 26% in 2004, while for female graduates the rates declined from 18.6% in 1977 to about 16.3% in 2004. At the 3-digit level within-firm occupational mobility rates are much lower (about 10–15%) than across-firm occupational mobility rates (about 25–50%). Female graduates exhibit less occupational mobility than male graduates – a result which has also been documented by Fitzenberger and Kunze (2005). Seibert (2007) also shows that in both groups occupational mobility is highest if leaving the training firm is accompanied by an unemployment spell. Furthermore, Seibert (2007) finds that occupational mobility rates vary by training occupation, and training occupation-specific occupational mobility rates have changed over time.

Given the growing awareness regarding the economic significance of occupational mobility, this empirical study contributes to the literature on worker mobility by providing a comprehensive overview of occupational mobility in the West German labor market over a 27-year-period. Based on a large administrative data set provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB), this analysis provides results on patterns of occupational mobility for a major group of West German workers, that is for those in full-time and part-time employment subject to social security contributions during the period 1982–2008. The paper also addresses potential explanations for the observed patterns of occupational mobility and presents descriptive evidence to assess the relevance of these explanations. Administrative data is generally thought to be very reliable, which reduces concerns regarding the quality of the occupational information. A further advantage of the data set used in this analysis is that it allows me to observe intermittent spells of non-employment, and I can therefore analyze patterns of occupational mobility in the context of more unstable career episodes marked by either (long-term) unemployment or long-term absence of workers from the data set.

Furthermore, the existence of firm-internal labor markets suggests that workers' occupational mobility behavior may differ in the firm-internal context. Indeed, those studies that explicitly report results on within-firm occupational mobility tend to find that within-firm occupational mobility rates are sizable, but are generally lower than across-firm occupational mobility rates (Groes *et al.* (2015); Moscarini and Thomsson (2007); Seibert (2007); Zangelidis (2008)).[1] Correspondingly, in a recent study on occupational mobility right

[1] For Denmark Groes *et al.* (2015) find that the average probability of an occupational switch is higher for firm-switchers than for those who stay with the firm. Nevertheless, they report that within-firm occupational mobility rates are of significant size. The authors' main finding that the probability of occupational mobility is u-shaped, and that occupational mobility is directional, applies to both types of mobility behavior. Also, Moscarini and Thomsson (2007) note that in the US over the period 1994–2006 about 40% of occupational switches occur while remaining with the current employer. Zangelidis (2008) reports that in Britain over the period 1991–2001 about 55% of workers have at least once switched occupations across firms, while

after graduation from apprenticeship based on German administrative data, Fitzenberger *et al.* (2015) show that amongst graduates the share of firm-and-occupation switchers is about twice as high as that of within-firm occupation switchers. Also, the causal wage effects of occupational mobility are very heterogeneous across groups. On the one hand, occupational switches within the training firm lead to large and persistent wage gains, and thus reflect a form of career progression. On the other hand, across-firm occupation switches at entry into the first job after graduation result in average wage losses. However, for those firm-and-occupation switches that are directed towards relatively better paid occupations, the wage effect is actually positive in the short run. Also, compared to pure firm switches there is no additional wage decrease due to an occupational switch across firms. Furthermore, Fitzenberger *et al.* (2015) show that the relation between occupational mobility and local labor market characteristics differs across the two types of occupational mobility. Within-firm occupational switches are related to push factors (e.g. high labor market tightness), while across-firm occupational switches are also related to push factors (e.g. high local unemployment rate). Overall, these findings corroborate the conjecture that across-firm and within-firm occupational mobility behavior may be driven by different factors, and may therefore exhibit different overall patterns.

Apart from the above-mentioned studies, to the best of my knowledge occupational switches within firms have so far received no explicit attention in the literature on occupational mobility.[2] Many studies that document patterns of occupational mobility either exclude occupational mobility within firms from the analysis or they include it in an overall occupational mobility measure.[3] This paper therefore further contributes to the literature on worker mobility by investigating patterns of occupational mobility separately across firms and within firms. Indeed, the analysis reveals strong differences between patterns of across-firm and within-firm occupational mobility.

The main results on the patterns of across-firm and within-firm occupational mobility in the West German labor market during the period 1982–2008 are as follows:

1. Across-firm occupational mobility in the West German labor market is high, with nearly half of all moves across establishments being accompanied by a simultaneous switch in the 3-digit occupation. On average, about one third of all across-firm

only about 51% have at least once switched occupations while staying with their employer. Interestingly, the results provided at the 3-digit level in Lalé (2012) imply that in France on average over the period 1982–2009 occupational mobility within employers was higher than occupational mobility across employers – see Lalé (2012) Table 2, p. 380.

[2] At the time this chapter of my doctoral thesis was last revised, I was supervising a Master thesis that was meant to analyze occupational mobility across and within firms based on data from the German Socioeconomic Panel. However, at the time of the last revision, the Master thesis was not yet available to me.

[3] For example, Moscarini and Thomsson (2007), Parrado *et al.* (2007), Kambourov and Manovskii (2008), Bachmann and Burda (2007) and Isaoglu (2010a,b) use an overall measure of occupational mobility, which contains occupational switches both across and within firms. Lalé (2012) uses an overall measure of occupational mobility that includes occupational switches with the same employer at the 3-digit level (as long as the job classification changes), but explicitly excludes within-firm occupation changes at the 1- and 2-digit levels of the analysis. Longhi and Brynin (2010) only consider across-employer occupational mobility in the main part of their analysis.

occupational switches occur across presumably large distances between four broad occupational groups. Within-firm occupational mobility rates are lower than across-firm mobility rates. On average, about 29% of all observed 3-digit occupational switches happen within firms.

2. Across-firm occupational mobility rates have significantly increased over the period 1982–2008 and exhibit a pro-cyclical pattern. Within-firm occupational mobility rates have significantly decreased over the observation period. They do not follow a clear cyclical pattern.
3. About 52% of all across-firm occupational switches constitute a change towards a generally better paid occupation. In the case of within-firm occupational switches the share of upward switches is even a bit higher (about 55%).
4. Patterns of across-firm occupational mobility vary across demographic subgroups. Most importantly, across-firm occupational mobility is decreasing in age. A thought experiment holding the demographic composition of employment fixed at its 1982 structure suggests that demographic change, especially the aging of the workforce, has worked against the positive trend in across-firm occupational mobility. In contrast to this, patterns of within-firm occupational mobility are fairly homogeneous across demographic subgroups. Correspondingly, no clear relationship between demographic change and within-firm occupational mobility patterns emerges.
5. Both the occupational composition as well as the industry composition of employment have changed since 1982 in a way that seems to have favored occupational mobility across firms. An analysis of the relation between changes in the occupational composition of the workforce and within-firm occupational mobility yields no clear results. However, the analysis suggests that the industry structure of employment has changed in a way that has dampened within-firm occupational mobility rates.
6. Overall gross occupational mobility rates are much larger than net occupational mobility rates, which suggests that in the West German labor market occupational mobility is more related to worker-job-mismatch than to changes in occupation-specific labor demand. In Western Germany net occupational mobility has not increased over time, which suggests that the observed increase in across-firm occupational mobility cannot be explained by increasing variation in occupation-specific labor demands over time.
7. Differentiating between different types of career episodes reveals that there is a positive relation between the incidence of unemployment and across-firm occupational switches. Surprisingly, even in long-term unemployment episodes about 48% of across-firm occupational switches are directed towards on average better paid occupations. Within-firm occupational mobility rates are relatively higher in the case of episodes marked by long-term absence from the data set. This could, for example, reflect cases in which firms send their employees abroad for an extended period, or cases in which

employees obtain extensive further vocational training. Across all types of career episodes, more than half of all occupational switches within firms are directed toward generally better paid occupations.

8. Patterns of across-firm and within-firm occupational mobility differ in many respects. While many of the results on across-firm occupational mobility are in line with what has been found for other countries, the documented patterns of within-firm occupational mobility are novel to the extent that the phenomenon has received little explicit attention in the literature so far.

The remainder of the paper is structured as follows: Section 3.2 describes the data, the sampling procedures and the quality of the occupational information. Section 3.3 describes the empirical approach and contains the main results of the analysis. These are supplemented by results on several potential explanations for the observed patterns of occupational mobility. Section 3.4 concludes. The Appendix 5.2 contains additional tables and figures (Tables and Figures starting with "A."), as well as further information regarding the robustness of the main results of the analysis.

3.2 Empirical Design and Data

3.2.1 Data

The empirical analysis is based on the Sample of Integrated Labor Market Biographies (SIAB) Scientific Use File (Regional file 7508) provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB). This is an administrative data set based on social security records which contains a large random sample of German employees and workers who worked at least once in a job subject to social insurance contributions sometime during the period 1975–2008. The data set contains the employment histories of about 1.5 million individuals, supplemented by data on their benefits receipt. Employment spells include information on the worker's current occupation and daily wages.[4] From the entirety of labor market biographies contained in the SIAB I draw a sample of employment spells at the 30th of June of each year. I thereby construct a worker-year panel that spans the period of 1981–2008.[5] This allows me to compare each worker's employment status and occupation between the current and the previous job.

Furthermore, I classify the person-year observations in the data set into different types of career episodes. Specifically, I perceive each person-year observation as marking the end of a career episode that began after the most recent 30th of June on which a worker was registered as being in employment. For each person-year observation I calculate the total number of days that have passed since the most recent person-year observation (i.e. the most recent

[4] See Dorner *et al.* (2011) for further information on the SIAB SUF Regional file 7508.

[5] According to the SIAB Handbook the benefit receipt records up to 1980 are incomplete (Dorner *et al.* (2011), p. 29), thus I discard all spells from the period 1975–1980 from the data set.

employment spell on a 30th of June).[6] This time gap has, by definition, a minimum length of 365 days, but it may also span several years. For the intermittent period (since the last 30th of June spent in employment) leading up to the respective person-year observation I also record the number of days spent in intermittent employment in a West German establishment, or in unemployment (benefits receipt), as well as the number of days the respective worker was temporarily missing from the data set (e.g. due to self-employment, further education, working abroad, etc.). I also record the number of days spent in intermittent employment in an East German establishment.

Table A.11 describes the different types of career episodes that I can identify based on the obtained information. The majority of person-year observations in the data set mark the end of a regular employment episode in West Germany with a length of one year and no intermittent unemployment as well as no intermittent employment in an East German establishment. Additionally, I can identify career episodes that are employment-dominated with less than a total of 365 days spent in unemployment, as well as career episodes that are dominated either by unemployment or by the worker's disappearance form the data set. I also classify a set of career episodes that involve intermittent employment in East German establishments, which will be excluded from the main sample of analysis.

3.2.2 Sample

The analysis focuses on full- and part-time workers in West Germany during the period 1982–2008.[7] Specifically, the main sample includes all types of career episodes that do not contain any intermittent employment in an East German establishment. This allows me to consider occupational mobility rates during different phases of a worker's labor market career, such as, for example, in the context of an episode of intermittent extensive unemployment. As mentioned above, the person-year panel is constructed in a way that a time gap between two person-year observations may span several years. I exclude from the analysis the complete career histories of all workers that contain at least one time gap longer than five years.[8]

The main sample contains both male and female prime-aged workers (20–60 years) working in the private sector. The SIAB contains no information on officials, but it contains information on workers employed in the public sector. However, the respective sectors are difficult to identify due to the aggregation of industries at the 2-digit level, so I only exclude those 2-digit industries that are most likely public.[9] Overall, these sampling criteria leave me

[6] For this, I split all spells covering the 30th of June into two spells: one that ends on the 30th of June and one that starts on the 1st of July.

[7] Appendix 5.2.1 provides selected results separately for full-time and part-time employment. The main results of the analysis are largely robust to excluding part-time employment from the sample.

[8] This sampling rule still leaves me with about 99% of person-year-observations and about 87% of workers at that step of sample reduction. It allows me to focus on a more homogeneous group of workers.

[9] I exclude the following 2-digit industries from the main sample: asylums, hospitals and education ("Heime, Krankenhäuser, Erziehung"), (street) cleaning, associations and organizations ("(Straßen)Reinigung, Verbände, Organisationen"), as well as public administration and social security ("Öffentliche Verwaltung, Sozialversicherung").

with a main sample of about 6.387.000 person-year observations, which corresponds to the careers of about 581.000 workers and an average number of about 235.000 observations per year.

3.2.3 Measuring Occupational Mobility

In Germany, employers have to register all employees subject to social security contributions with the social insurance agencies. The social insurance agencies also have to be notified of subsequent changes in the employment status. The information reported to the social insurance agencies also includes information on the worker's occupation, which the employer has to identify using a 3-digit occupational code in accordance with the 1988 "Classification of Occupations. Systematic and Alphabetical Directory of Job Titles" published by the Federal Employment Agency. Thus, for each worker contained in the data set I can observe his occupational career as long as it took place in employment subject to social security contributions.

Employment spells in the SIAB SUF contain occupational information in the form of 120 different 3-digit codes[10], which correspond to 29 broader occupational categories at the 2-digit-level and 4 large occupational groups at the 1-digit-level, respectively. Table A.12 gives an overview of the occupational classifications used at the 1- and 2-digit levels. I identify occupational mobility by comparing the occupational codes of the current and the previous job. Furthermore, I can identify whether in his current job a worker is employed at a different establishment than in his previous job. This allows me to differentiate between occupational mobility across and occupational mobility within establishments.[11]

3.2.4 Quality of Occupational Information

A widespread concern in the study of occupational mobility is the extent of measurement error. Most importantly, do changes in occupational codes truly reflect changes in the set of tasks performed by a certain worker or are they just a result of imprecise coding?

Some scholars perceive changes in occupational codes that occur within the same firm (with the same employer) as relatively unreliable (see e.g. Longhi and Brynin (2010), Lalé (2012)). However, while most of the related studies on occupational mobility typically use survey data, the SIAB is an administrative data set which is generally thought to be very reliable. The occupational code is not the core component of the information reported by the employer to the social insurance agencies, since it is not directly relevant to the calculation of social security contributions and pension payments. Nevertheless employers are required to

[10] In the SIAB Scientific Use File the original 3-digit occupational codes have been aggregated into 120 3-digit occupations for reasons of anonymization.

[11] In a slight abuse of notation, I use the terms "firm" and "establishment" interchangeably, although the SIAB data actually only contains information at the establishment level. Thus, the SIAB allows me to identify mobility across establishments, which need not always imply mobility across firms.

fill in the respective forms diligently and truthfully. The employer has full information on the kind of job his employee performs, and therefore miscoding of occupations is presumably less likely than in the case of survey-based data collection. Furthermore, as long as a worker stays with a certain employer, occupations are, in a sense, dependently coded[12] and in the SIAB miscoding should be especially unlikely for within-firm occupational mobility. Against this background, in this paper I view changes in occupational codes that occur within an establishment as valuable information and I do not discard within-firm occupational changes as spurious. In the empirical analysis I differentiate between two kinds of occupational mobility: within firms and across firms.

If anything, it is more plausible that occupational mobility could be under-reported within firms. As Bachmann and Burda (2007), p. 6 point out, some occupational switches with the same employer (those where only the occupational affiliation changes), only have to be reported to the social insurance agencies as part of a mandatory notification at the end of the year. As I compare the occupational information available on the 30th of June of each year, this means that some of the within-firm occupational switches that occur within the second half of the intermittent twelve months might not be captured by the within-firm occupational mobility measure. However, as long as the respective workers do not reverse the occupational change over the following few months, such unreported switches should at least be captured by the within-firm occupational mobility measure in the subsequent year. Another concern could be that measures of within-firm occupational mobility could be affected by firm-specific singular events such as personnel turnover in the human resources department of firms or the introduction of administration software used to digitize the reporting process. There is indeed anecdotal evidence of such singular events that result in spikes in occupational mobility rates within single firms. Unfortunately, the data used in this study does not allow for the identification of firms (or firm-fixed effects), to correct for such events. However, as long as these events are unsystematic, this should not affect the main results of this study. This holds especially in light of the fact that I observe a decrease, not an increase in within-firm occupational mobility rates over the period of observation.

To further assess the quality of the occupational information in the SIAB data, I check the incidence of missings in the occupational code variable, and I check whether the share of missing occupational information has changed over time. As Table A.13 shows, the average share of missings in the occupation variable is about 1.68% for the raw data containing only employment spells. When I draw the person-year-panel and then gradually apply sampling rules, the share of missings decreases. If I apply the full set of sampling rules used for the

[12] Several studies on occupational mobility emphasize the advantages of dependently coded occupations relative to independently coded ones. Both Kambourov and Manovskii (2008) and Parrado *et al.* (2007) are based on the Panel Study of Income Dynamics (PSID) covering the 1970s to 1990s. The main problem of the data lies in a structural break in occupational coding procedures. Mobility rates are found to be lower when based on retrospective coding in contrast to the original (independent) coding procedure. Moscarini and Thomsson (2007) use monthly CPS data for 1979–2006. They rely on dependent coding and identification of atypical career (mobility) patterns for dealing with the problem of measurement error. Using the French Labor Force Survey Lalé (2012) also limits the analysis to workers with dependently coded occupations.

main analysis, except those rules that are related to missings in the occupation variable, the average share of missings in the occupation variable reduces to about 0.17%. Furthermore, as Figure A.11 illustrates, the yearly share of missings in the occupation variable has increased over the period 1982–2008. When I apply all sampling rules except those that are related to missings in the occupation variable, I find lower shares of missing occupational information, but the positive trend does not vanish.

A concern might be that the increase in the share of missing occupational information may indicate that employers have over time become less careful with reporting their workers' 3-digit occupational code, so that additional to the increase in missing information also the accuracy of the reported occupational codes has decreased over time. While I cannot test this directly, it is reassuring that the main patterns of occupational mobility documented in the paper pertain to the broad 1-digit-level as well as to the more detailed 2- and 3-digit levels. Occupational changes at the 1-digit-level should be most robust to measurement error, since they imply drastic changes in occupation, and coding errors should more likely pertain to the occupational details than to the broad occupational category.

To alleviate the concern that the main results regarding trends in occupational mobility could be related to the increasing share of missings in the occupation variable, in Section 3.3.3 I perform a set of robustness checks. These show that even if all missings in the occupation variable (which translate into missings in the occupational mobility indicators) are imputed as occupational changes (or, in contrast, as the occupation remaining stable), the main results regarding the time trends in occupational mobility rates do not change qualitatively.

3.3 Occupational Mobility in the West German Labor Market

In this section I present the main results on the patterns of occupational mobility in the West German labor market for the period 1982–2008, including a set of robustness checks regarding the increasing share of missings in the occupation variable. I also consider how my results relate to other empirical studies both for Germany and internationally. I then continue by discussing several potential explanations for the observed trends in occupational mobility, and I provide descriptive results to assess the relevance of these potential explanations.

3.3.1 Empirical Approach

To assess the extent of occupational mobility in the West German labor market I estimate yearly gross mobility rates separately for within- and across-firm occupational mobility using ordinary least squares (OLS) estimation. Specifically, I create two dummy indicators I_t^{across} and I_t^{within} that are equal to 1 if a person in a given year switched occupations compared to her most recent person-year observation in the sample and at the same time left the previous

establishment (across-firm occupational mobility) or stayed with the employer (within-firm occupational mobility), respectively. I also create a dummy indicator $I_{it}^{firmswitch}$ that is equal to 1 if a person switched firms compared to the most recent person-year observation, irrespective of whether an occupational change occurred simultaneously.

To obtain yearly mobility rates I then separately regress each dummy indicator on a full set of year dummies (without a constant):

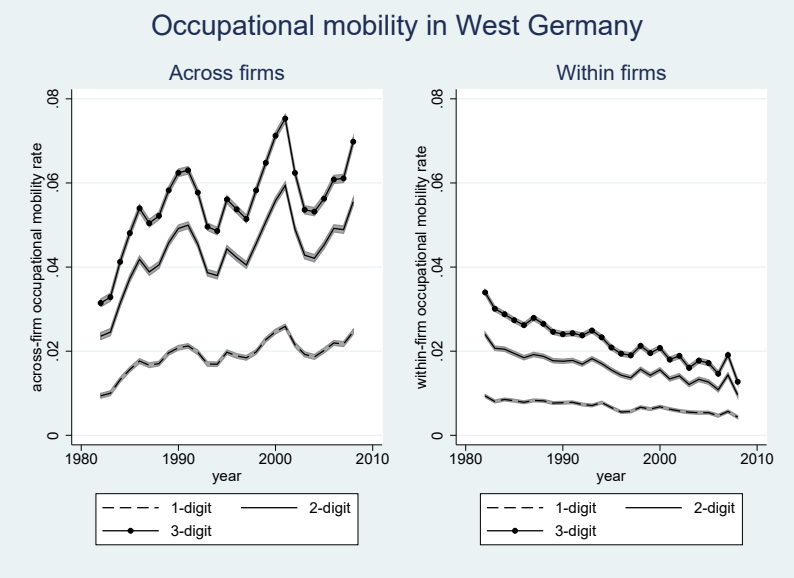
$$\begin{aligned} I_{it}^{across} &= \beta_0 \cdot year_{1982} + ... + \beta_{27} \cdot year_{2008} + u_{it} \\ I_{it}^{within} &= \beta_0 \cdot year_{1982} + ... + \beta_{27} \cdot year_{2008} + u_{it} \\ I_{it}^{firmswitch} &= \beta_0 \cdot year_{1982} + ... + \beta_{27} \cdot year_{2008} + u_{it} \end{aligned}$$

Thus, each estimated coefficient can be interpreted as a yearly mobility rate that gives the share of workers in overall employment in a given year who were mobile along the defined dimensions relative to their most recent person-year observation. To obtain detailed mobility rates for subgroups, e.g. by gender or education, I interact the respective sets of dummy indicators with the year dummies in the OLS regressions. Furthermore, to obtain trends in the time series of occupational mobility rates, I regress the time series of log-occupational mobility rates on a constant and a linear trend variable.

Note that if a person-year observation cannot be used for the analysis due to the sampling rules, the subsequent person-year observation is also not contained in the final sample on which the estimations are performed. That is, if a person-year observation in t-2 had to be excluded from the analysis (e.g. due to a missing in the occupation variable, employment in the public sector, etc.), the next observation in t-1 also has to be excluded from the analysis. Or phrased differently, a person has to be observed for at least one period after an observation had to be removed from the sample, before it (re-)enters the sample. However, the information in t-1 can still be used to code the mobility indicators in t. That is, the dummy indicators are actually coded before the sampling rules are applied to minimize the overall number of missings in the occupational mobility variables.

To assess whether West German workers tend to move towards better or worse paid occupations, I estimate occupation-year-specific average wages by running OLS regressions on a sample of West German full-time workers for the period 1982–2008. Separately for each of the 120 3-digit occupations, I regress workers' real wages on a full set of year dummies (no constant included). The coefficients on the year-dummies reflect the average wages by occupation in the respective year. Within each year I then rank all 3-digit occupations ordinally according to their average wage from lowest (rank 1) to highest (rank 120). I differentiate between upward and downward occupational switches by checking whether the rank of the new 3-digit occupation in the current period is above or below the rank of the original occupation in the previous period. This approach to comparing occupational ranks over time leaves me with a small number of observations where no change in the ordinal rank occurs. Specifically, 0.86% of across-firm occupational switches and 0.83% of within-firm

Figure 3.1: Occupational Mobility over the Period 1982–2008 in West Germany



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

occupational switches cannot be classified as either upward or downward, but are in a sense "flat". Because these shares are very low and very similar for the two different types of occupational mobility, I omit them from the figures and from the discussion in the results section.

3.3.2 Main Results

Figure 3.1 shows the evolution of estimated occupational mobility rates within and across firms in West Germany. Table 3.1, Panel A, p. 46 reports average mobility rates across the period 1982–2008. At the 3-digit level across-firm occupational mobility rates rose from about 3.2% in 1982 to about 7% in 2008, with an average of about 5.6%. At the 2- and 1-digit levels the respective averages are about 4.4% and 1.9%. So about one third of all across-firm occupational switches occur across presumably large distances between four broad occupational groups. As Figure A.12 shows, overall mobility across establishments (irrespective of occupational changes) has also increased from about 7.3% in 1982 to about 13.8% in 2008 with an average rate of 11.8%. Thus, on average nearly half of all switches across establishments (47%) are accompanied by a switch in the 3-digit occupation.

Turning to occupational mobility within firms, Figure 3.1 provides a very different picture. At the 3-digit level within-firm occupational mobility rates have decreased from about 3.4% in 1982 to about 1.3% in 2008, with an average of about 2.2%. At the 2- and 1-digit levels the respective averages are about 1.6% and about 0.7%, respectively. That is, a bit less than one third of all within-firm occupational switches occur across four broad occupational groups.

Even if one adds up across-firm and within-firm occupational mobility rates to make the

Table 3.1: Average Yearly Mobility Rates (%) over the Period 1982–2008 in West Germany

	occupational mobility across firms	occupational mobility within firms	mobility across firms	net occupational mobility
	A. Main analysis			
1-digit	1.90	0.68		0.46
2-digit	4.36	1.61		0.77
3-digit	5.55	2.23		1.15
			11.75	
	B. Hypothetical mobility rates: demographic change (base = 1982)			
1-digit	1.97	0.69		
2-digit	4.42	1.64		
3-digit	5.53	2.24		
			11.56	
	C. Hypothetical mobility rates: occupational composition (base = 1982)			
1-digit	1.84	0.68		
2-digit	4.25	1.63		
3-digit	5.42	2.24		
			11.42	
	D. Hypothetical mobility rates: industry composition (base = 1982)			
1-digit	1.82	0.71		
2-digit	4.17	1.66		
3-digit	5.29	2.27		
			11.20	

results more comparable to the measure of overall occupational mobility employed in other studies, it is apparent that the rates of occupational mobility in West Germany reported above are much lower than what has been found by Kambourov and Manovskii (2008) for the US, and also lower than what Longhi and Brynin (2010) report for Britain. It seems that the magnitude of occupational mobility rates in West Germany is more similar to what has been found by Lalé (2012) for France. For West Germany, Bachmann and Burda (2007), Longhi and Brynin (2010), and Isaoglu (2010a,b) tend to find lower rates of overall occupational mobility than this study does, but compared to the results on the US and Britain, the results on West Germany are all in the same range of magnitude. However, one has to keep in mind, that a comparison of the extent of occupational mobility across studies, and even more so across countries, is inherently difficult, since these studies differ with respect to the underlying institutional framework, the data generating process, the sampling restrictions, the nature of the occupational classification used in the analysis, and the construction of the occupational mobility variables.

Inspection of Figure 3.1 indicates that while across-firm and within-firm occupational mobility rates were at similar levels in 1982, they have evolved contrarily over the subsequent 27-year period. Table 3.2, Panel A shows the linear trends in log mobility rates. I find a statistically significant positive time trend for across-firm occupational mobility rates, while rates of occupational mobility within firms have statistically significantly decreased over time. At the 3-digit level the across-firm occupational mobility rate has increased by an average 1.7% per year, with even slightly larger average growth at the 1- and 2-digit levels. During the same period, at the 3-digit level the within-firm occupational mobility rate has

Table 3.2: Trends in Log Mobility Rates over the Period 1982–2008 in West Germany

	occupational mobility		mobility across firms
	across firms	within firms	
	A. Main analysis		
1-digit	0.0222***	-0.0233***	
2-digit	0.0189***	-0.0246***	
3-digit	0.0170***	-0.0276***	
			0.0180***
	B. Hypothetical mobility rates: demographic change (base = 1982)		
1-digit	0.0292***	-0.0224***	
2-digit	0.0246***	-0.0232***	
3-digit	0.0215***	-0.0268***	
			0.0207***
	C. Hypothetical mobility rates: occupational composition (base = 1982)		
1-digit	0.0186***	-0.0246***	
2-digit	0.0159***	-0.0246***	
3-digit	0.0144***	-0.0273***	
			0.0155***
	D. Hypothetical mobility rates: industry composition (base = 1982)		
1-digit	0.0180***	-0.0205***	
2-digit	0.0144***	-0.0223***	
3-digit	0.0123***	-0.0258***	
			0.0137***

Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of log(yearly mobility rates). Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

decreased by an average 2.8% per year. Here, the decrease is a bit less pronounced at the 1- and 2-digit levels. All trend estimates are statistically significant at the 1% level. In addition, Table A.14, Panel B shows the trend estimates in the case of an alternative quadratic trend specification, while for ease of comparison Panel A repeats the linear trend estimates from Table 3.2, Panel A. While the quadratic term is statistically insignificant in the trend regressions for within-firm occupational mobility, it is small but statistically significant in the case of across-firm occupational mobility. While in these regressions the linear term is even more pronounced, it appears that the increase in across-firm occupational mobility has slowed down a bit over time. This also holds for the results on pure job-to-job mobility. Most importantly however, the main results of an increase in across-firm occupational mobility and a decrease in within-firm occupational mobility are robust to these alternative specifications. As shown in Appendix 5.2.1, these main results also hold if we exclude part-time workers from the analysis as a further robustness check. An increase in overall occupational mobility (comprising across-firm and within-firm occupational mobility) has also been found for the US (Kambourov and Manovskii (2008); Parrado *et al.* (2007); Moscarini and Thomsson (2007)) and – once the author corrects for the dampening influence of the aging of the French workforce – also by Lalé (2012) for France. Isaoglu (2010a,b) observes no trend in overall occupational mobility over the period 1984–2004 in West Germany based on data from the German Socioeconomic Panel, but also provides no trend estimation results. Bachmann and Burda (2007), p. 15 state that they observe no long-run trend in overall occupational

mobility over the period 1980–2000, but inspection of Figure A.4 in Bachmann and Burda (2007), p. 31 actually suggests that the fraction of employment-to-employment transitions that involved a 3-digit occupational switch has risen since the early 1980s.

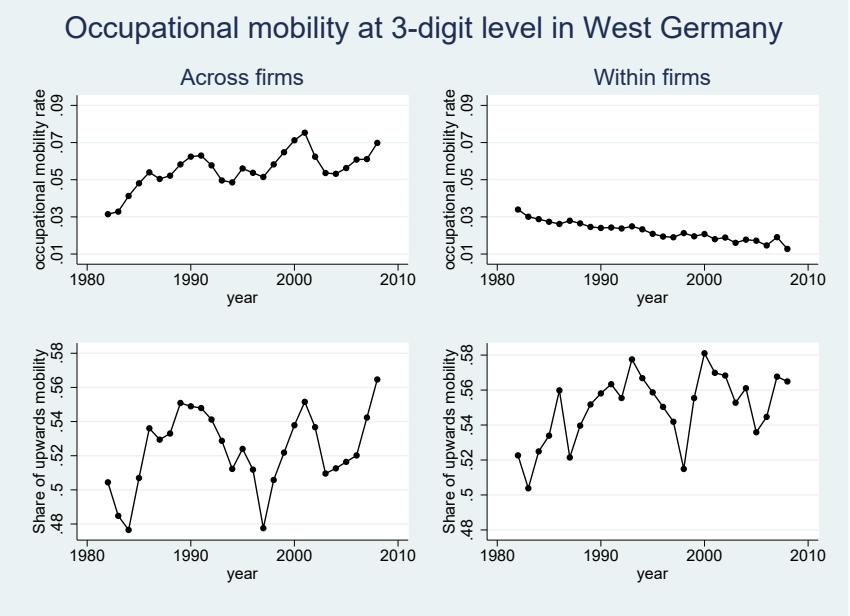
While a detailed analysis of the cyclicality of occupational mobility rates in the West German labor market is beyond the scope of this paper, Figure 3.1 gives an impression of the cyclical pattern of across-firm occupational mobility rates. For a basic assessment, Figure A.13 plots the 3-digit across-firm occupational mobility rate as well as GDP growth. Graphical inspection suggests that across-firm occupational mobility is pro-cyclical (with a lag of about one year). Pro-cyclical patterns of an overall measure of occupational mobility have been documented for the US by Kambourov and Manovskii (2008), Moscarini and Thomsson (2007), and by Moscarini and Vella (2008), for France by Lalé (2012), as well as for West Germany by Isaoglu (2010a,b).[13] Also for West Germany, Bachmann and Burda (2007) observe that gross overall occupational mobility involving an employment-to-employment transition is pro-cyclical. As Figure A.14 shows, overall firm-to-firm mobility also appears to be pro-cyclical. Meanwhile, according to Figure A.15 the 3-digit within-firm occupational mobility rate shows no clear cyclical pattern.

From a theoretical point of view the individual wage effects of occupational mobility are ambiguous, and empirical studies have found contrasting empirical evidence for both positive and negative wage effects of occupational mobility, suggesting that the wage effect of occupational mobility depends on the context of the mobility decision. The focus of this paper is to provide an overview of patterns of occupational mobility in West Germany, and I do not estimate the causal wage effects of occupational mobility. Still, the following descriptive analysis may provide us with an impression of the typical direction of occupational switches. As described in section 3.3.2, I rank occupations according to their average wages, and then differentiate between upward- and downward mobility accordingly.

On average over the period 1982–2008 about 52.3% of across-firm occupation changers move towards a higher ranked occupation, while within firms about 55% are upward changes. The result that a large proportion of occupational switches is directed towards relatively better paid occupations fits well with Longhi and Brynin (2010)'s finding that in Germany occupational mobility across employers is associated with an increase in wage growth and job satisfaction. In another study based on data from the German Socioeconomic Panel Nisic and Trübswetter (2012) report that about 52% of 2-digit occupational switches over the period 1994–2008 are voluntary (e.g. having been promoted, having switched jobs out of own accord, etc.), and that the respective occupational switches are associated with average wage gains. Interestingly, the share of voluntary occupational switches documented in Nisic and Trübswetter (2012) is quite similar in magnitude to the shares of upward occupational mobility reported above. Furthermore, a similar pattern emerges for Denmark, where Groes *et al.* (2015) show that both within and across firms a relatively higher proportion of

[13] While Lalé (2012) uses an overall measure of occupational mobility, the author reports that the cyclical pattern of occupational mobility is mostly driven by occupational mobility across employers.

Figure 3.2: Occupational Mobility at the 3-digit Level and Shares of Upward Mobility



occupational switches are directed towards on average better paid occupations.[14]

Figure 3.2 shows the development of the share of upward moves in across-firm occupational mobility as well as the share of upward moves in within-firm occupational mobility over the observation period. In the case of across-firm occupational mobility the share of upward moves follows a roughly pro-cyclical pattern. As discussed above, the within-firm mobility rate shows no cyclical pattern. However, the share of upward moves in within-firm occupational mobility exhibits a roughly pro-cyclical pattern. Figure 3.2 thus suggests that during economic upswings workers are, on average, more likely to move towards better paid occupations. This observation pertains both to the West German labor market as well as to the internal labor markets of West German establishments.

3.3.3 Robustness of Trend Estimates

In this section I present robustness checks to alleviate the concern that the main results regarding trends in occupational mobility could be related to the increasing share of missings in the occupation variable. When I draw the person-year-panel and then apply the full set of sampling rules used for the main analysis, except those rules that are related to missings in the occupation variable, the average yearly share of missings in the occupation variable is about 0.17%. These missings in the occupation variable translate into missings in the occupational mobility indicators, since every time a missing in the occupation variable occurs, the mobility indicators in the respective, as well as in the subsequent period, cannot be computed.

As a robustness check, I create two benchmark scenarios for 3-digit occupational mobility.

[14] Compare the fractions of firm stayers and firm switchers moving upward or downward, respectively, in Groes *et al.* (2015), Table A2.

Table 3.3: Robustness Check: Average Mobility Rates and Trends in Log Mobility Rates at the 3-digit Level over the Period 1982–2008 in West Germany when Making Different Assumptions on the Meaning of Missings in the Occupation Variable

	occupational mobility	
	across firms	within firms
	A. Main results: Missings excluded	
average rate	5.55	2.23
time trend	0.0170 ***	-0.0276 ***
	B. Upper benchmark: Missings counted as mobility	
average rate	5.62	2.38
time trend	0.0183 ***	- 0.0192 ***
	C. Lower benchmark: Missings NOT counted as mobility	
average rate	5.53	2.22
time trend	0.0167 ***	-0.0278 ***

Trend estimates: Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of log(yearly mobility rates) for the person-year panel with full set of sampling rules (except in Panels B and C sampling rules related to missing in the occupation variable not applied). Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

For the upper benchmark I impute all missings in the occupational mobility indicators as mobility (i.e. setting the indicator to "1"). The imputation also takes account of whether a change of establishment occurred at the same time. That is, if the occupational mobility indicators contain a missing, and the firm-to-firm mobility indicator indicates a firm switch, I recode the across-firm mobility indicator to "1", and at the same time set the within-firm occupational mobility indicator to "0". Furthermore, I create a lower benchmark case where all missings in both occupational mobility indicators are imputed as zero mobility. I then compute yearly occupational mobility rates both for the upper benchmark case and the lower benchmark case, which I plot in Figures A.16 and A.17. Finally, I estimate the linear time trends of these time series.

Table 3.3 shows the estimation results and allows us to compare the two benchmark scenarios to the main results presented in Section 3.3.2. The main results are based on a sample from which cases with missing occupational codes and missings in the occupational mobility indicators have been excluded. According to Table 3.3, Panel B, if the missings in the mobility indicators would be interpreted as actual cases of occupational mobility, the linear trend in across-firm occupational mobility would be more pronounced, while the decline in within-firm occupational mobility would be smaller. In contrast, if we interpret all missings in the occupation variable as if no occupational mobility had occurred, the trend coefficients change only slightly relative to the main results presented in Panel A. Most importantly, the results in Table 3.3 illustrate that the main insights gained from the linear trend estimations are qualitatively robust to this imputation experiment at the 3-digit level.

3.3.4 Demographics and Occupational Mobility

The results presented in Section 3.3.2 show that in the West German labor market occupational mobility rates across firms have significantly increased since the beginning of the 1980s, while within-firm occupational mobility rates have significantly decreased over the same period. In the following I show how these patterns translate to certain subgroups of the workforce, and I show how the observed patterns of occupational mobility are related to changes in the demographic structure of employment. Indeed, other studies on occupational mobility for the US and France indicate that the demographic change which has occurred in Western economies over the past decades seems to have dampened the evolution of occupational mobility rates (Kambourov and Manovskii (2008), Lalé (2012)).

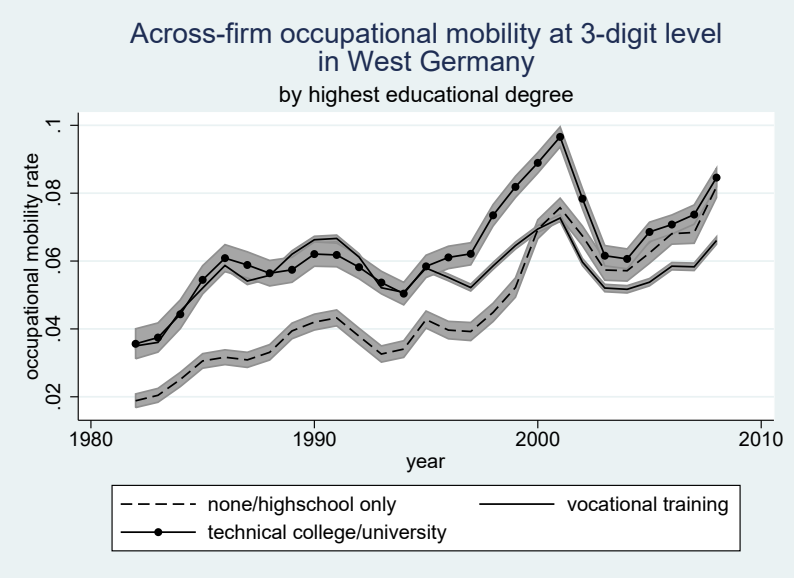
Demographic Subgroups

In the following I plot occupational mobility rates for certain subgroups of West German workers. These results are supplemented by Table A.15 which shows average yearly occupational mobility rates by demographic subgroup, as well as by Tables A.16–A.18 which show the respective trend estimates at the 3-digit level.

Figures 3.3 and 3.4, p. 52 show the evolution of 3-digit-level occupational mobility rates by the highest educational degree obtained. Across firms low-skilled workers have been less occupationally mobile at the beginning of the observation period, but the gap relative to medium- and high-skilled workers has closed over time. Interestingly, medium- and high-skilled workers exhibit very similar patterns of across-firm occupational mobility until the spike for high-skilled workers occurs around the year 2000. For a measure of overall occupational mobility Isaoglu (2010a) also finds that higher educational attainment is associated with higher rates of occupational mobility in West Germany. In the case of within-firm occupational mobility the picture is less clear. Except for a few spikes in the within-firm occupational mobility rate of high-skilled workers the patterns for all three education groups are very similar (and not statistically different from each other).

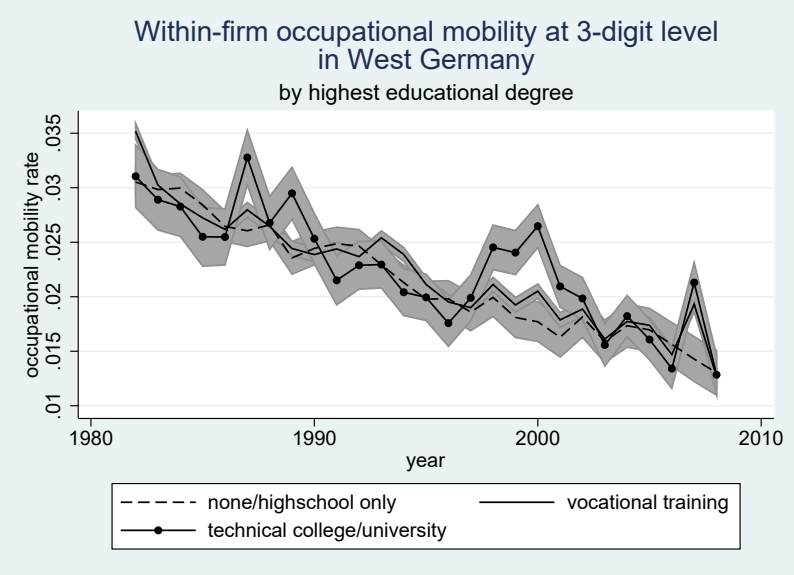
Figures A.18 and A.19 show occupational mobility rates at the 3-digit-level for gender-education-cells. In the case of across-firm occupational mobility male and female workers show quite similar dynamic patterns of occupational mobility. Highly educated female workers show a bit higher occupational mobility rates than their male counterparts – while the opposite holds for low- and medium-skilled female workers. Figure A.19 shows that within firms low- and medium-skilled female workers tend to be less likely to change their occupation relative to male workers. Both genders show parallel decreasing trends in time, though. Isaoglu (2010a) finds that in West Germany overall occupational mobility rates are a bit higher for female workers once non-employment-to-employment transitions are also taken into account, but the study unfortunately does not provide results for gender-education-cells. In contrast to this, Bachmann and Burda (2007) report that overall occupational mobility in

Figure 3.3: Across-firm Occupational Mobility at the 3-digit Level by Highest Educational Degree



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure 3.4: Within-firm Occupational Mobility at the 3-digit Level by Highest Educational Degree



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

West Germany is a bit lower among female workers relative to male workers.

Figures A.20 and A.21 show occupational mobility rates at the 3-digit-level for age-education-cells. According to Figure A.20 the across-firm occupational mobility is decreasing in age. This negative relationship has also been documented for a measure of overall occupational mobility by Kambourov and Manovskii (2008), Parrado *et al.* (2007), Bachmann

and Burda (2007), Isaoglu (2010a,b) and Lalé (2012). Interestingly, Figure A.21 reveals almost no difference in within-firm occupational mobility across age groups.

The described differences in the patterns of occupational mobility across demographic subgroups of workers are consistent with the average yearly occupational mobility rates at the detailed level of gender-education-age-group cells provided in Table A.15 as well as the trend estimates provided at the 3-digit level in Tables A.16 and A.17. While the trend estimates vary across subgroups, the main result of a statistically significant increase in across-firm occupational mobility and a statistically significant decrease in within-firm occupational mobility during the period 1982–2008 holds across all demographic subgroups of workers (with the trends in across-firm occupational mobility in the relatively small group of female workers aged 55–60 being the exception). Also, as Table A.18 shows, firm-to-firm mobility has statistically significantly increased over time in all subgroups.

Inspection of Figures A.18-A.21 reveals that within-firm mobility patterns appear to be much more homogeneous across subgroups than in the case of across-firm occupational mobility. This difference between within- and across-firm occupational mobility suggests that different factors drive the occupational mobility behavior of workers depending on whether the occupational switch takes place within the current establishment or not.

Demographic Composition

I continue the analysis of the relationship between the demographic structure of employment and patterns of occupational mobility in the West German labor market by turning to the role of demographic change. Holding the demographic structure of employment fixed at its 1980 values, Kambourov and Manovskii (2008) find that without demographic change the increase in occupational mobility would ceteris paribus have been even more pronounced in the US. For France Lalé (2012) finds a positive trend in across-firm occupational mobility only once he corrects for the demographic change that occurred since the 1980s. The author shows that occupational mobility is decreasing in age, and identifies the natural aging of the French workforce as the main driver of demographic composition effects. Following Kambourov and Manovskii (2008) and Lalé (2012) I pose a similar question for Germany: What if the demographic composition of employment had remained constant since the year 1982?

Table 3.4, p. 54 illustrates how the demographic composition of the main sample has changed over the period 1982–2008 with respect to age, gender as well as the educational achievements of the workforce. The share of female workers has increased by about 12% since 1982. While over the period of observation the average age of the workers in the sample has not changed much, the share of young workers has decreased and the share of middle-aged workers has increased. Table 3.4 also shows the educational upgrading that has taken place since 1982.

For each gender-education-age-group cell I calculate its yearly weight as the share of the respective worker group in overall employment. I then calculate hypothetical mobility rates

Table 3.4: Demographic Composition of Employment over the Period 1982–2008 in West Germany (Shares in %)

	1982	1980s	1990s	2000s
female	29.72	30.51	32.87	33.29
age group				
20–24	8.93	9.81	6.70	4.77
25–34	24.58	26.03	30.60	24.90
35–44	29.22	25.82	27.60	33.42
45–54	27.20	28.50	24.43	27.37
55–60	10.07	9.84	10.68	9.55
mean age (years)	40.09	39.72	39.63	40.66
highest educational degree				
no degree/high school only	22.05	18.03	12.79	10.04
vocational training	73.41	76.60	79.91	79.17
technical college/university	4.54	5.37	7.30	10.79

Descriptive statistics are calculated as averages over the respective years mentioned in column title (exception: 1980s based on years 1983–1989 only).

as a weighted average of cell-specific mobility rates using the demographic shares of the gender-education-age-group cells in 1982 as weights. Based on the hypothetical mobility rates (base = 1982), I calculate average yearly mobility rates and I estimate the linear trends in hypothetical occupational mobility rates. For further illustration I follow Lalé (2012) in calculating indices which allow me to visualize the differences between the hypothetical mobility rates that are based on the year 1982's demographic structure and the actual mobility rates:

$$I_t^{mr} = \frac{mr_t^{actual} - mr_t^{base=1982}}{mr_t^{actual}}$$

with *mr* indicating the respective occupational mobility rate. Based on these indices Figures 3.5-3.6 illustrate the ceteris paribus relationship between changes in the demographic composition and occupational mobility rates.[15]

According to Table 3.1, Panel B the hypothetical average yearly mobility rates over the period 1982–2008 are very similar to the actual ones. As Table 3.2, Panel B shows, this also holds for the hypothetical linear trend estimates in the case of within-firm occupational mobility. However, the hypothetical linear trends are more pronounced than the actual ones in the case of across-firm occupational mobility.

Figure 3.5 suggests that by 2008 the 3-digit across-firm occupational mobility rate would have been about 8% higher if the overall demographic structure had not changed since 1982. Considering indices that are calculated separately for changes in the age (gender, education) composition, it appears that the gender composition has been neutral in this overall development. The change in the educational composition has actually been favorable to across-firm occupational mobility for most of the period, while since the late 1990s the

[15] It makes little difference whether I choose the year 1982 or the 1980s as base period, and the conclusions do not change qualitatively if I use the 1990s or 2000s as an alternative base period.

Figure 3.5: Demographic Composition and Across-firm Occupational Mobility at the 3-digit Level

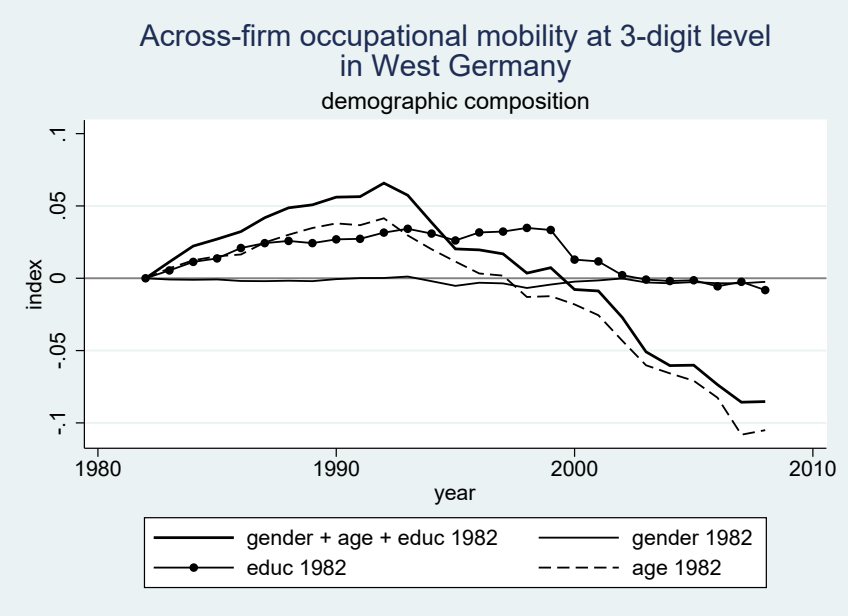
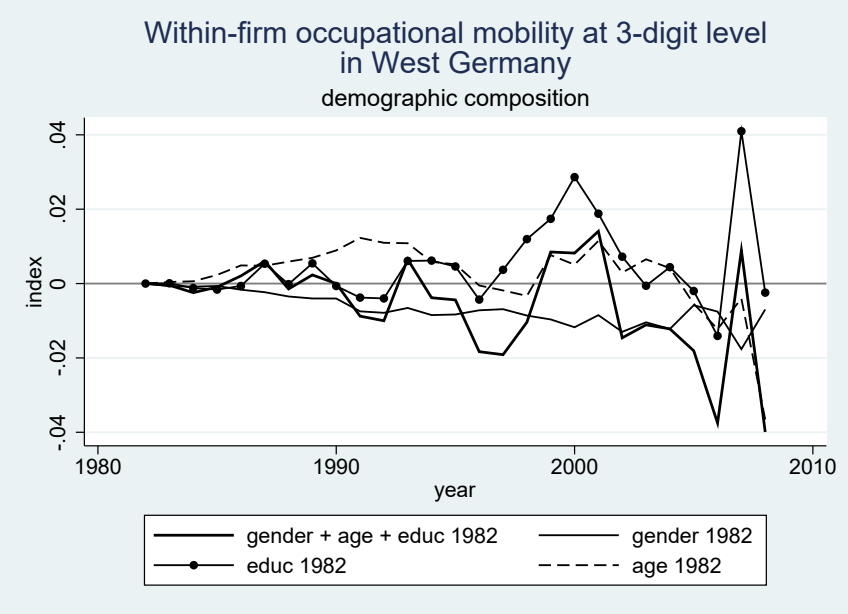


Figure 3.6: Demographic Composition and Within-firm Occupational Mobility at the 3-digit Level



change in the age structure of employment seems to have dampened across-firm occupational mobility. According to Figure 3.5, across-firm occupational mobility would have been about 10% higher in 2008 had the aging of the workforce not taken place since the 1970s. Considering across-firm occupational mobility at the 1-digit and 2-digit levels, the results are even more pronounced. By 2008 the 1-digit (2-digit) across-firm occupational mobility rate would have been about 15% (13%) higher if the overall demographic structure of employment

had not changed since 1982.

Regarding the relation between demographic change and 3-digit within-firm occupational mobility the picture provided by Figure 3.6 is not clear, since the indices are very volatile. It seems that within firms changes in the gender composition have not been neutral in the overall development, but are rather related to a dampening of occupational mobility. Changes in the age structure seem to have been partly favorable for within-firm occupational mobility. If I consider 1-digit and 2-digit within-firm occupational mobility, the results are similar. The fact that no clear picture emerges for the composite index is consistent with the result that patterns of within-firm occupational mobility are very homogeneous across demographic subgroups. Again, this suggests that occupational mobility behavior is driven by different factors depending on whether it takes place within the firm or across firms.

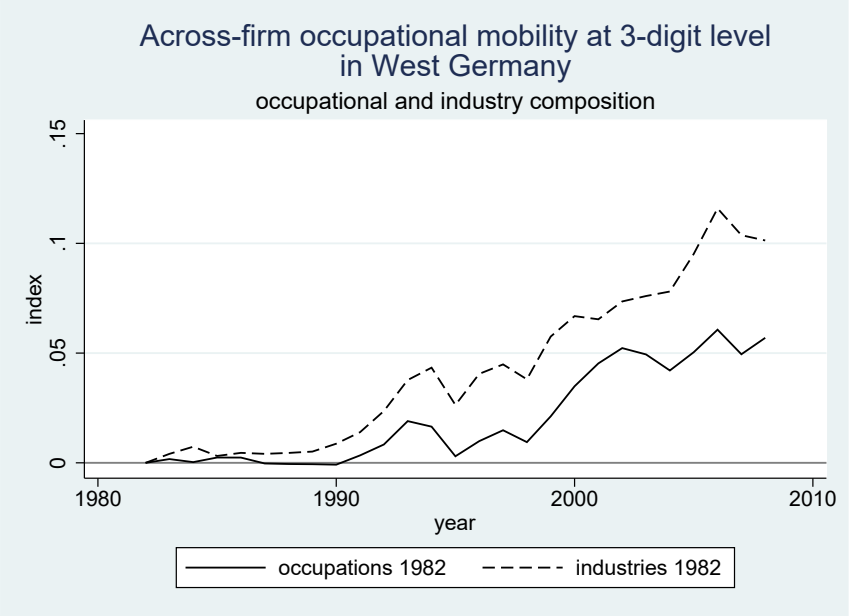
Overall, the results on the relation between the demographic composition of employment and occupational mobility in the German labor market suggest that the observed trends in occupational mobility rates since the beginning of the 1980s have not been driven by demographic change – especially in the case of across-firm occupational mobility.

3.3.5 Changes in Occupational and Industry Structure

Apart from demographic change the composition of West German employment could also have changed along other dimensions which could be associated with the observed trends in occupational mobility rates. Both the occupational composition and the industry composition of employment could have changed since the beginning of the 1980s in ways that favor (or dampen) occupational mobility. For example, Seibert (2007) finds that in Germany occupational mobility rates at first labor market entry after graduation from apprenticeship vary by training occupation. He also reports that training-occupation-specific occupational mobility rates have changed over time, with occupational mobility out of some training occupations increasing, and out of others decreasing over the period 1977–2004. Furthermore, Bachmann and Burda (2007) argue that sectoral and occupational turbulence as measured by Lilien indices increased in the mid 1990s in West Germany. They also report that on average over the period 1976–2000 more than half of all sectoral switches were accompanied by an occupational switch at the 3-digit level. In the remainder of this section, I therefore explore the relation between the occupation/industry composition of employment and patterns of occupational mobility.

To analyze the relationship between occupational mobility and the occupation structure of employment, as well as the industry structure of employment, I conduct a thought experiment similar to the one described in Section 3.3.4 for the case of demographic change. That is, I keep the occupational (industry) composition of employment fixed to the base period 1982 and then construct hypothetical mobility rates which reflect how occupational mobility rates had evolved ceteris paribus had the composition of employment remained constant since

Figure 3.7: Occupational/Industry Composition and Across-firm Occupational Mobility at the 3-digit Level



the year 1982.[16] Again, I calculate average yearly mobility rates and I estimate the linear trends in hypothetical occupational mobility rates. I also calculate indices which allow me to visualize the differences between the hypothetical mobility rates that are based on the year 1982's occupation (industry) employment structure and the actual mobility rates.

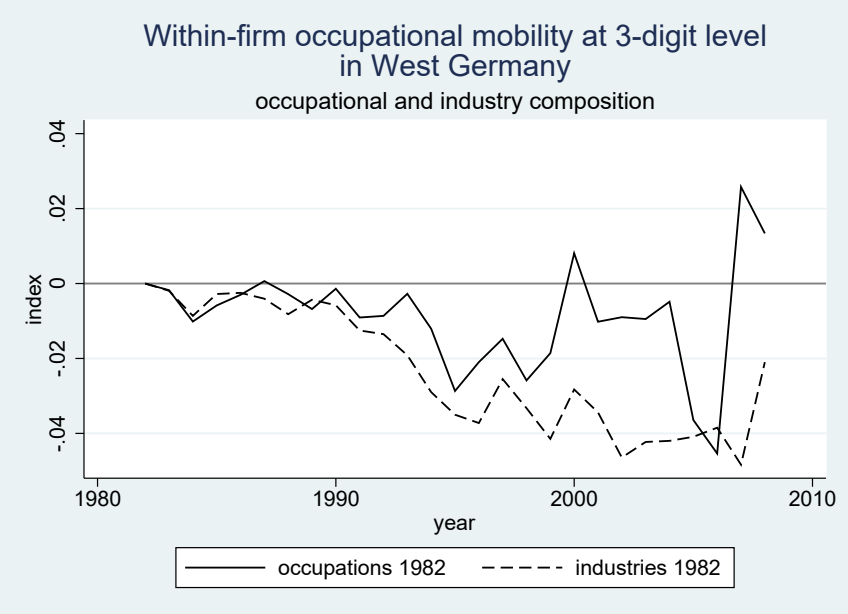
Regarding the occupational composition of employment, according to Table 3.1, Panel C the hypothetical average yearly mobility rates over the period 1982–2008 are lower than the actual ones in case of across-firm occupational mobility, but rather similar in the case of within-firm occupational mobility. As Table 3.2, Panel C shows, this also holds for the hypothetical linear trend estimates in the case of across-firm occupational mobility. Within firms the hypothetical negative linear trends are more pronounced than the actual ones at the 1-digit level, but less pronounced at the 3-digit level.

Figure 3.7 suggests that by 2008 the 3-digit across-firm occupational mobility rate would have been about 5% lower had the occupational structure of employment not changed since 1982. At the 1-digit (2-digit) level the hypothetical gap in 2008 is even higher with 8% (7%). This suggests that since the beginning of the 1980s the occupational composition of employment in the West German labor market has changed in a way that favors across-firm occupational mobility.

Turning to within-firm occupational mobility the picture provided in Figure 3.8, p. 58 is, as in Section 3.3.4, less clear, since the index is again very volatile. By 2008 the 3-digit within-firm occupational mobility rate would have been about 2% higher had the occupational

[16] Again, it makes little difference whether I choose the year 1982 or the 1980s as base period. Also, the main conclusions do not change qualitatively if I use the 1990s or 2000s as an alternative base period.

Figure 3.8: Occupational/Industry Composition and Within-firm Occupational Mobility at the 3-digit Level

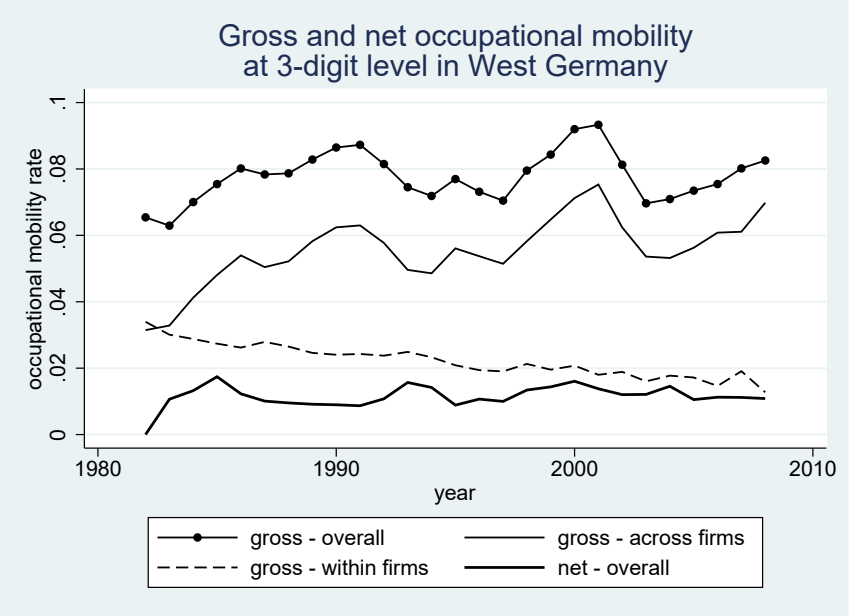


structure of employment not changed since 1982, but the index actually remains below zero for most of the period leading up to 2008. The results are similarly inconclusive at the 1-digit and 2-digit level.

Interestingly, I obtain a much clearer picture for the relation between changes in the industry composition of employment and occupational mobility rates. As Table 3.1, Panel D shows the hypothetical average yearly mobility rates over the period 1982–2008 are lower than the actual ones in case of across-firm occupational mobility, but they are higher than the actual ones in the case of within-firm occupational mobility. Also Table 3.2, Panel D shows that the hypothetical linear trend estimates are less pronounced than the actual ones both in the case of across-firm as well as in the case of within-firm occupational mobility.

Accordingly, Figure 3.7 suggests that by 2008 the 3-digit across-firm occupational mobility rate would have been about 10% lower had the industry structure of employment not changed since 1982. I obtain similar results for the 1-digit and 2-digit levels. In contrast to this, Figure 3.8 suggests that by 2008 the 3-digit within-firm occupational mobility rate would have been about 2% higher had the industry structure of employment not changed since 1982. The gap is even larger for most of the period since the mid 1990s, and it is also larger at the 1-digit and 2-digit levels (4% and 3% in 2008, respectively). This suggests that since the beginning of the 1980s the industry composition of employment in the West German labor market has changed in a way that has favored across-firm occupational mobility, but at the same time has dampened within-firm occupational mobility.

Figure 3.9: Gross and Net Occupational Mobility at the 3-digit Level



3.3.6 Net Occupational Mobility

The main results described in Section 3.3.2 of this paper are based on gross occupational mobility rates. In addition to this, we can also gain insights into the factors driving the patterns of occupational mobility by considering net flows of workers across occupations. To assess the extent of variation in the employment shares of occupations at the 3-digit level, I calculate a yearly measure of net occupational mobility following Kambourov and Manovskii (2008):

$$\text{net mobility}_t = \tfrac{1}{2}\textstyle\sum_m |s_{m,t} - s_{m,t-1}|$$

with $s_{m,t}$ being the share of employment in 3-digit occupation m in overall employment in year t. This measure reflects the extent of changes in the relative size of occupations.

According to Table 3.1, Panel A the average net occupational mobility rate over the period 1982–2008 was about 1.2% at the 3-digit level. Figure 3.9 shows the development of both overall gross and net occupational mobility at the 3-digit level. The extent of net occupational mobility has been rather stable over the period 1982–2008. A trend regression for log net occupational mobility at the 3-digit level yields a very small positive (0.0052), but statistically insignificant linear trend estimate. The trend regressions at the 1-digit and 2-digit level also yield small statistically insignificant estimates. Additionally, Figure A.22 shows that net occupational mobility exhibits a cyclical pattern at all digit levels.

As Figure 3.9 shows, the overall gross occupational mobility rate (the sum of across-firm and within-firm occupational mobility rates) is much higher than the net occupational mobility rate. That is, we observe a lot of mobility across occupation cells, and these

flows can at best be only partially attributed to average changes in the relative employment shares of occupations over time. Similar observations have been made by Kambourov and Manovskii (2008) for the US, by Lalé (2012) for France, and by Isaoglu (2010b) for West Germany. According to Jovanovic and Moffitt (1990)'s reasoning regarding worker mobility across industries, and according to what Kambourov and Manovskii (2008) summarize in the context of occupational mobility, such a ratio of gross and net occupational mobility suggests that the occupational mobility which we observe rather stems from the worker-occupation matching process than from shifts in occupation-specific labor demand. The results on net occupational mobility found in the above-mentioned studies imply that so-called "churning" (the difference between gross and net occupational mobility) amounts to about 40% of gross occupational mobility at the 3-digit level in the US[17], and to about two thirds in France (see Lalé (2012)). For Western Germany, based on data from the German Socioeconomic Panel, Isaoglu (2010b) reports that annual gross occupational mobility at the 4-digit level averages about 5%, while net occupational mobility averages about 2.7% over the period 1984–2004. The results in Isaoglu (2010b) imply that churning amounts to about 46% of gross occupational mobility.[18] Based on IAB administrative data, I find an even more pronounced result. At the 3-digit level occupational churning (at a rate of 6.6%) corresponds to about 85% of overall gross occupational mobility.

The view that in the West German labor market occupational mobility could be a result of worker-occupation-mismatch is also in line with Longhi and Brynin (2010)'s finding that in West Germany the probability of an occupational switch across employers is positively related to workers being either over- or under-qualified for the current job. Interestingly, since in Western Germany net occupational mobility has not increased over time, this suggests that the observed increase in across-firm occupational mobility cannot be explained by increasing variation in occupation-specific labor demands over time. This contrasts with what Kambourov and Manovskii (2008) conclude for the US.

3.3.7 Occupational Mobility and Unemployment

The literature on occupational mobility stresses the relevance of the labor market context in which the occupational switch occurs. Some authors differentiate between voluntary switches, which are interpreted as a way to improve the worker-job-match, or a form of career progression, and involuntary switches due to a worker being (or becoming) unemployed (see e.g Longhi and Brynin (2010), Nisic and Trübswetter (2012)). Given the pro-cyclical pattern of across-firm occupational mobility described in Section 3.3.2, it appears plausible that at least the patterns of across-firm occupational mobility in the West German labor market should be related to the incidence of unemployment. Furthermore, the findings in

[17] For Kambourov and Manovskii (2008) I calculate the respective share based on the gross and net occupational mobility rates reported in the paper.

[18] Isaoglu (2010b), p. 11 states that churning "accounts for about one quarter of the total reallocation." However, given the average numbers on gross and net occupational mobility cited above, I do not understand how the author arrives at this result.

Table 3.5: Average Yearly Mobility Rates (%) by Type of Career Episode and Average Share of Career Episodes in Main Sample (%) over the Period 1982–2008 in West Germany

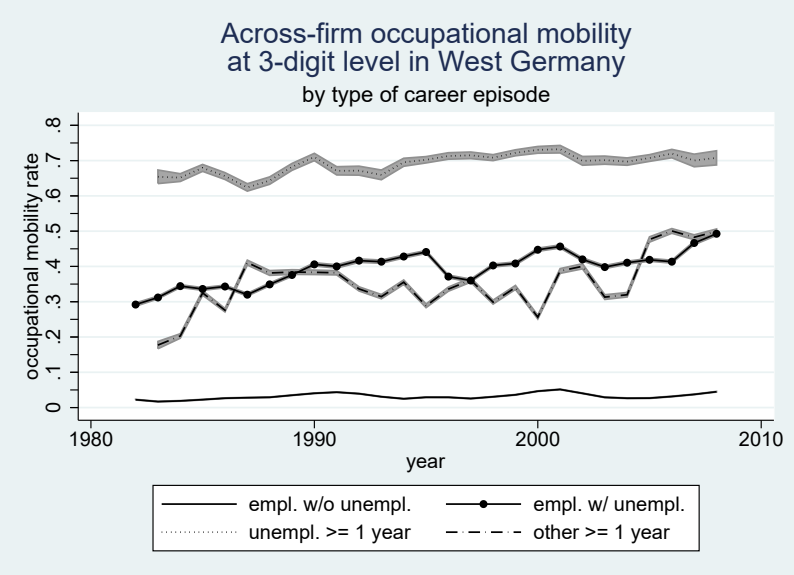
	occupational mobility		mobility across firms	share in sample
	across firms	within firms		
A. Employment in West Germany without interm. unemployment				
1-digit	1.02	0.68		
2-digit	2.43	1.61		
3-digit	3.20	2.22		
			8.36	93.76
B. Employment in West Germany with interm. unemployment				
1-digit	13.65	0.47		
2-digit	31.51	1.13		
3-digit	39.42	1.61		
			63.35	3.91
C. Intermittent days in unemployment $>=$ 365 days				
1-digit	28.07	0.84		
2-digit	57.83	1.50		
3-digit	69.06	1.97		
			91.47	0.62
D. Intermittent days missing from data set $>=$ 365 days				
1-digit	14.36	1.23		
2-digit	29.69	3.08		
3-digit	35.35	4.15		
			52.43	1.80

Bachmann and Burda (2007) and Isaoglu (2010a) show that labor market episodes that involve unemployment, and even more so unemployment-to-employment transitions involve relatively higher rates of overall occupational mobility. In this section, I therefore explore the relation between the incidence of unemployment during career episodes and the associated patterns of occupational mobility.

As described in Section 3.2.1, I can identify different types of career episodes that may occur throughout a worker's career. As shown in Table 3.5 the main sample contains four types of career episodes:

- About 93.8% of person-year observations in the sample mark the end of a regular *employment episode* in West Germany with a length of typically one year and no intermittent unemployment (and less than one year of absence from the data set).
- Additionally, I identify career episodes that are employment-dominated with less than a total of 365 days spent in unemployment, which I refer to as *short-term unemployment episodes*. These constitute about 3.9% of person-year observations in the sample.
- The sample also contains career episodes that are dominated either by unemployment of a total length of more than 365 days (*long-term unemployment episodes*), or by the workers absence from the data set for a total of more than 365 days (*long-term missing episodes*). About 0.6% of person-year observations mark the end of a *long-term unemployment episode*, while about 1.8% of person-year observations mark the end of a *long-term missing episode*.

Figure 3.10: Across-firm Occupational Mobility at the 3-digit Level by Type of Career Episode



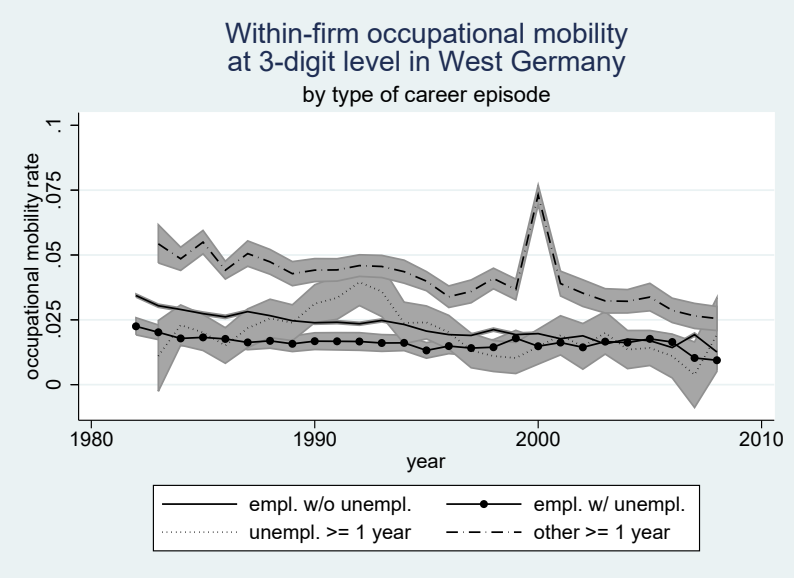
Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Remember that the main sample contains only career episodes without any intermittent employment in an East German establishment. Note also, that *employment episodes* typically have a length of one year (about 97% of *employment episodes*), and an average length of 1.03 years. About 66% of *short-term unemployment episodes* have a length of one year, and the average length of this type of career episode is 1.4 years. The other two types of career episodes can have a length of two to five years.[19] *Long-term unemployment episodes* have an average length of 3.20 years, whereas *long-term missing episodes* haven an average length of 2.96 years.

While the composition of the main sample is dominated by the large share of *employment episodes*, Table 3.5 reveals the heterogeneity of average occupational mobility rates across different types of career episodes. Most importantly, as Figure 3.10 clearly illustrates, average across-firm occupational mobility rates differ strongly across career episode types. For regular *employment episodes* the respective average mobility rates are lower than the overall values presented in Section 3.3.2 at all digit levels. In contrast to this, all other types of career episodes exhibit very high levels of across-firm occupational mobility. At the 3-digit level on average about 39% of *short-term unemployment episodes* entail an occupational switch across firms. In the case of *long-term unemployment episodes* the average across-firm occupational mobility rate even amounts to about 69%. Finally, on average *long-term missing episodes* entail an across-firm occupational switch in about 35% of cases. Of course, the exit from unemployment need not always entail an occupational switch, but these results

[19] As has been explained in Section 3.2.2, the full career histories of workers with at least one career episode longer than five years have been dropped from the sample. This allows me to focus on a more homogeneous group of workers.

Figure 3.11: Within-firm Occupational Mobility at the 3-digit Level by Type of Career Episode



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

indicate that there exists a plausible positive relation between the incidence of (long-term) unemployment and across-firm occupational switches.[20] The observation that occupational mobility rates are higher if intermittent unemployment is allowed for in the sample has also been documented for the US by Kambourov and Manovskii (2008) and for West Germany by Isaoglu (2010a). My results are also in line with Bachmann and Burda (2007), who also use IAB administrative data and show that relative to employment-to-employment transitions overall occupational mobility rates over the period 1980–2000 are higher if they consider non-employment-to-employment transitions, and are especially high if they consider unemployment-to-employment transitions. Additionally, Table 3.5 shows that pure firm-to-firm mobility rates are also relatively higher for *unemployment episodes* and *long-term missing episodes.*

Turning to patterns of within-firm occupational mobility for different types of career episodes as shown in Figure 3.11, we observe a more homogeneous picture. The average within-firm occupational mobility rates computed for *employment episodes* are very close to the overall values presented in Section 3.3.2, and the average within-firm occupational mobility rates computed for *short-term unemployment episodes* and *long-term unemployment episodes*

[20]Note that when an occupational change is recorded at the end of, for example, a *long-term unemployment episode*, this does not necessarily imply that the exit from unemployment occurred simultaneously with the switch of occupation. For example, the unemployed may have exited unemployment by taking up work in his original occupation, and then later during the career episode may have switched to a new occupation (before the 30.6. of the respective year), that would then be recorded at the end of the respective career episode. However, given the strong heterogeneity of average across-firm occupational mobility rates across career episode types, the positive relationship between unstable career episodes and the incidence of occupational mobility is striking.

also do not differ much from the overall results. Interestingly, within-firm occupational mobility rates are relatively higher in the case of *long-term missing episodes*. This could, for example, reflect cases in which firms send their employees abroad for an extended period, or cases in which employees obtain extensive further vocational training or a master's degree.

Does the heterogeneity in average mobility rates across different types of career episodes, especially in the case of across-firm occupational mobility, also translate into differences in the evolution of mobility rates over the period 1982–2008? When I estimate the linear trends in log mobility rates by type of career episode as shown in Table A.19, I find that the main result of a positive trend in across-firm occupational mobility as well as a negative trend in within-firm occupational mobility is robust to focusing solely on the dominant group of *employment episodes* (see Panel A). Actually, the trends are even a bit more pronounced than those reported in Section 3.3.2. This observation holds at all digit levels of occupational mobility.

In the case of across-firm occupational mobility, Table A.19 reveals that the positive trend estimates are comparably smaller for *short-term unemployment episodes*. This is even more apparent in the case of *long-term unemployment episodes*, where the trend estimate amounts to a yearly growth of only 0.4% at the 3-digit level. However, the positive trend estimates for *long-term missing episodes* are more pronounced than the results presented in Section 3.3.2 for the overall sample.[21] There is also some heterogeneity in trend estimates for within-firm occupational mobility. The negative trend estimates are comparably much less pronounced in the case of *short-term unemployment episodes*, and also less pronounced in the case of *long-term missing episodes*. However, in the case of *long-term unemployment episodes* the negative trend estimates are more pronounced than the results presented in Section 3.3.2 for the overall sample.

Could the overall trends in occupational mobility documented in Section 3.3.2 be driven by changes in the share of *unemployment episodes* and *missing episodes* in the sample? The fact that the main results regarding the trends in occupational mobility are robust to limiting the analysis to *employment episodes* already suggests that this is not the case. In addition to this, I regress the log yearly sample share by type of career episode on a trend variable and a constant. I find no trends in the share of all four types of career episodes contained in the main sample.[22] That is, on average the composition of the sample has not changed with respect to the type of career episodes. This shows that the positive trend in across-firm occupational mobility is not driven by an increased presence of long-term unemployment

[21] Graphical evidence provided in Figure A.4 in Bachmann and Burda (2007), p. 31 shows that the rate of occupational mobility conditional on leaving unemployment increased over the period 1980–2000, while the same does not hold when one conditions on employment-to-employment transitions instead. Note, however, that the empirical approach taken in Bachmann and Burda (2007) differs from my approach in several ways. For example, the authors exploit the spell-structure of the data to identify employment transitions cumulatively. That is, they cumulatively capture most of a worker's employment transitions within a year, whereas I compare information on workers between the 30th of June of each year. Furthermore, Bachmann and Burda (2007) explicitly study transitions out of employment or unemployment, whereas I partly consider career episodes that contain employment mixed with intermittent episodes of unemployment.

[22] Detailed estimation results are not reported, but are available from the author upon request.

Table 3.6: Average Yearly Share of Occupational Switches (%) by Direction and Type of Career Episode at the 3-digit Level over the Period 1982–2008 in West Germany

	occupational mobility across firms	occupational mobility within firms
A. Employment in West Germany without interm. unemployment		
upward	54.68	55.09
downward	44.48	44.08
B. Employment in West Germany with interm. unemployment		
upward	48.86	51.34
downward	50.28	47.62
C. Intermittent days in unemployment >= 365 days		
upward	47.97	60.37
downward	51.07	39.23
D. Intermittent days missing from data set >= 365 days		
upward	53.32	55.00
downward	45.71	44.25

episodes in the data, neither does it seem to be driven by an increased presence of long-term missing episodes in the data. A similar conclusion applies to the overall trend in within-firm occupational mobility.

Taking account of different types of career episodes has revealed a heterogeneity in the average rates of occupational mobility, especially in the case of across-firm occupational switches. In the following final step of the analysis, I further analyze the economic context of occupational mobility behavior by considering whether workers tend to switch towards on average better or worse paid occupations. As Table 3.6, Panel A shows, when we focus on regular *employment episodes* the shares of upward mobility are very similar for both across-firm and within-firm occupational mobility – both upward shares amount to about 55%.

I have shown that across-firm occupational mobility rates are very high in *short-term unemployment episodes* and *long-term missing episodes*. At the first glance, this suggests that the respective occupational mobility decisions occur under difficult individual circumstances. Under such conditions individuals may be more inclined to leave their original occupation and accept work in a relatively worse-paid occupation. In contrast to this conjecture, Table 3.6, Panel C shows that even in the case of *long-term unemployment episodes* about 48% of the across-firm occupational switches are still directed towards on average better paid occupations. The respective shares are even a bit higher for *short-term unemployment episodes* and *long-term missing episodes*. This is a surprising result which paints a much more positive picture of the relation between less stable career episodes and across-firm occupational switches. This interpretation is also in line with Longhi and Brynin (2010)'s finding that in Germany occupational mobility across employers is associated with an increase in wage growth and job satisfaction – even if times out of employment of up to one year are allowed for and cases of involuntary job switches are included in the sample. It needs to be mentioned, however, that as Figure A.23 shows, the yearly share of upward across-firm occupational switches in *long-term unemployment episodes* has strongly decreased since the

beginning of the 1990s and was at about 37% in 2008. For all other types of career episodes, including *short-term unemployment episodes* the share of upward across-firm occupational switches exhibits no apparent time trend.

When considering occupational mobility within firms, across all types of career episodes more than half of all occupational switches are directed towards generally better paid occupations. This is in line with the view that on the firm-internal labor market occupational switches may reflect promotions. The share of upward switches is especially high in the case of *long-term unemployment episodes*, but this should not be over-interpreted. The corresponding number of person-year-observations is rather small and as Figure A.24 shows the respective yearly share is very volatile.

3.4 Conclusions

This paper adds to the literature on worker mobility by providing a comprehensive overview of the extent and the dynamics of occupational mobility in the West German labor market over the period 1982–2008. Using a large administrative data set I differentiate between across-firm occupational mobility and within-firm occupational mobility. The analysis of mobility patterns yields contrasting results for these two types of occupational mobility.

Across-firm occupational mobility in the West German labor market is high, and a bit more than half of all across-firm occupational switches constitute a change towards an on average better paid occupation. Across-firm occupational mobility rates have significantly increased over the observation period and exhibit a pro-cyclical pattern. Regarding potential explanation of the observed mobility patterns, the analysis suggests that demographic change, especially the aging of the workforce, has worked against the positive trend in across-firm occupational mobility. In contrast to this, both the occupational composition as well as the industry composition of employment have changed since 1982 in a way that seems to have favored occupational mobility across firm. Furthermore, the analysis reveals a positive relation between the incidence of unemployment and across-firm occupational switches, which, however, cannot explain the positive trend in across-firm occupational mobility. Surprisingly, even in episodes marked by long-term unemployment an average of about 48% of across-firm occupational switches are directed towards on average better paid occupations. The fact that gross occupational mobility rates are much larger than net occupational mobility rates suggests that in the West German labor market occupational mobility is more related to worker-job-mismatch than to changes in occupation-specific labor demand. Net occupational mobility has not increased over time, which suggests that the observed increase in across-firm occupational mobility cannot be explained by increasing variation in occupation-specific labor demands over time.

For within-firm occupational mobility the empirical analysis provides a very different picture. Although across-firm and within-firm occupational mobility rates were at similar

levels in 1982, they have evolved contrarily afterwards. Within-firm occupational mobility rates have significantly decreased over the period 1982–2008, and they do not follow a clear cyclical pattern. About 55% of all within-firm occupational switches constitute changes towards on average better paid occupations. Patterns of within-firm occupational mobility are fairly homogeneous across demographic subgroups, and no clear relationship between demographic change and within-firm occupational mobility emerges. The relation between the occupational composition of the workforce and within-firm occupational mobility remains similarly unclear. However, the industry structure of employment seems to have changed in a way that has dampened within-firm occupational mobility rates. Furthermore, within-firm occupational mobility rates are relatively higher in episodes containing a long-term absence from the data set. This could, for example, reflect cases in which firms send their employees abroad for an extended period, or cases in which employees obtain extensive further vocational training.

A comparison of the extent of occupational mobility across studies, and even more so across countries, is inherently difficult, since studies differ with respect to the institutional framework, the data generating process, sampling restrictions and the exact nature of the occupational classification used in the analysis. However, one can compare overall patterns and trends, and many of the above-described results on across-firm occupational mobility in the West German labor market are in line with what other studies have found for Germany and other western economies. For example, a significant increase in occupational mobility has also been found for the US and France. Previous studies on West Germany that use measures of overall occupational mobility have not reported a positive trend in overall occupational mobility, but its pro-cyclical pattern has been documented for the US, France and West Germany. It is also a common finding that overall occupational mobility is decreasing in age, and demographic change has been found to have had a dampening relation to occupational mobility rates both in the US and France.

The finding that within-firm occupational mobility rates are lower than across-firm occupational mobility rates has also been reported in related studies for Germany and for other countries such as Denmark, Britain, and the US. Apart from this observation, the findings for within-firm occupational mobility presented in this paper are mostly novel in the sense that this phenomenon has received little explicit attention in the literature on occupational mobility so far. This paper assesses some potential explanations for the observed patterns of occupational mobility, and especially for the observed time trends. However, the chosen potential explanations such as, for example, changes in the demographic, occupation or industry composition of employment seem to fit the observed patterns of across-firm occupational mobility much better than the patterns of within-firm occupational mobility. The observed differences between across- and within-firm occupational mobility suggest that different factors drive these two types of occupational mobility behavior. This conclusion calls for further investigation of the two phenomena – and especially of the latter one.

A promising approach to further deepen our understanding of occupational mobility within

firms could be to investigate whether the occupational variety within firms has decreased over time so that the potential for occupational switches within firms has decreased as well. For example, this development could be the result of an increase in outsourcing of certain business units (also affecting the mobility behavior of high-skilled employees) or of non-focal services such as the operation of firm canteens, facility management, cleaning services, etc. (most likely affecting the mobility behavior of low-skilled employees). Indeed, in a recent study for Germany Goldschmidt and Schmieder (2015) find that domestic outsourcing of services in the areas of security, logistics, and cleaning as well as of food services has significantly increased over the period 1975–2008. However, such a more detailed analysis on the links between within-firm occupational variety and within-firm occupational mobility would require the use of linked employer-employee data.

The use of linked employer-employee data would also be conducive to the study of within-firm occupational mobility in an additional way. It would allow to check and, if indeed necessary, to correct for firm-specific spikes in occupational mobility caused by firm-specific singular events such as personnel turnover in the human ressources department or the introduction of new administration software. However, the additional use of linked employer-employee data is beyond the scope of this paper and should be conducted in a future research project.

4 Product Market Deregulation and Labor Market Outcomes in the German Skilled Crafts and Trades

4.1 Introduction

In the economic literature, cross-country differences in the evolution of employment have often been explained by differences in labor market regulation.[1] More recently, economic research points to product market regulation and limited product market competition as another important institutional factor that influences labor market outcomes such as employment. This view goes as far back as Krueger and Pischke (1997), who in their analysis of the U.S. employment miracle emphasize the relevance of product market regulations for the evolution of employment in developed economies. According to economic theory, product market regulations can create barriers to firm entry, which deter new firms from entering the market. This can have distorting effects on competition, price levels, and labor market outcomes such as wages and employment. Several papers argue that product market deregulation should have positive long-run effects on entrepreneurship and employment or, likewise, negative effects on unemployment. Fonseca *et al.* (2001) stress that start-up costs are an impediment to entrepreneurship. Modeling the choice between becoming an entrepreneur or a worker, they show that lower start-up costs should be associated with a higher number of entrepreneurs and, correspondingly, higher job creation rates and lower unemployment. Taking account of both product and labor market regulation in a general equilibrium model of the economy, Blanchard and Giavazzi (2003) show that through increased competition among firms, and a corresponding decrease in economic rents, product market deregulation has positive long-term effects on overall employment and wages. Spector (2004) also highlights the link between a higher degree of product market competition and higher employment, but points out that wages may be persistently lower. Ebell and Haefke (2009) rationalize a negative effect of product market deregulation on unemployment rates within a matching framework with frictions.

One strand of the economic literature that empirically analyzes the effects of product market regulation on labor market outcomes is based on cross-country comparisons.[2] In

[1] For an overview of the respective literature see Blau and Kahn (1999), as well as Nickell and Layard (1999) and Boeri (2011).

[2] For a less recent, but extensive overview of the cross-country evidence see also Schiantarelli (2008).

their widely-cited empirical study, Djankov *et al.* (2002) compare the degree of start-up entry regulations across 85 countries. They find that countries with stricter entry regulations exhibit more corruption, a larger shadow economy, and lower compliance with product quality rules and social standards. Lower levels of firm entry regulation are associated with more democratic forms of government, and generally better governance. Empirical studies with a focus on entrepreneurship show that stricter firm entry regulations are associated with reduced entry of new firms (Klapper *et al.* (2006); Ciccone and Papaioannou (2007); Dreher and Gassebner (2013); Klapper and Love (2014)), larger size of entrants (Klapper *et al.* (2006)), and slower growth in value added per employee of new firms (Klapper *et al.* (2006)). Stricter entry regulations are associated with higher within-industry concentration, and they also affect industry dynamics. Industries with stricter entry regulations react to growth opportunities through an increase in the size of existing firms, while less regulated industries tend to react through the entry of new firms (Fisman and Allende (2010)). In addition, cross-country studies with a focus on employment outcomes find that stricter product market regulations are associated with lower employment rates (Nicoletti and Scarpetta (2005); Feldmann (2008); Fiori *et al.* (2012)) and higher unemployment rates (Feldmann (2008); Freund and Rijkers (2014)). Countries with less strict business regulations are also more likely to undergo periods of sustained reductions in unemployment (Freund and Rijkers (2014)). Several studies show that the employment effects of product market deregulation interact with, and actually may get reinforced by the current degree of labor market regulations within a country (e.g. Nicoletti and Scarpetta (2005); Griffith *et al.* (2007); Fiori *et al.* (2012)). That is, product market deregulation is associated with larger positive (negative) effects on employment (unemployment) if labor market regulations are tight.[3] Furthermore, Griffith *et al.* (2007) find that product market deregulation and the resulting increase in competition is also associated with higher real wages.

While the above-mentioned cross-country studies undertake steps to neutralize potentially biasing influences, the concern remains that the countries under comparison may systematically differ with respect to characteristics that are not captured by the respective research designs. This concern is, for example, corroborated by Djankov *et al.* (2002)'s finding that stricter product market regulation is more common in countries with less democratic forms of government, worse governance, as well as higher levels of corruption. The country-level link between the prevalence of corruption and the existence of stricter product market regulations is also highlighted by Dreher and Gassebner (2013), who show that corruption may be a means to mitigate the effectiveness of barriers to firm entry in practice. It is therefore plausible that countries may systematically differ with respect to other unobserved characteristics which are related to the outcomes of interest. This may then bias the estimated labor market effects of product market (de-)regulation in cross-country studies. Against this background, over the past decade a new strand of empirical literature has developed which uses micro

[3] See Fiori *et al.* (2012) for a more extensive overview of the literature on the interaction effects between product and labor market regulations.

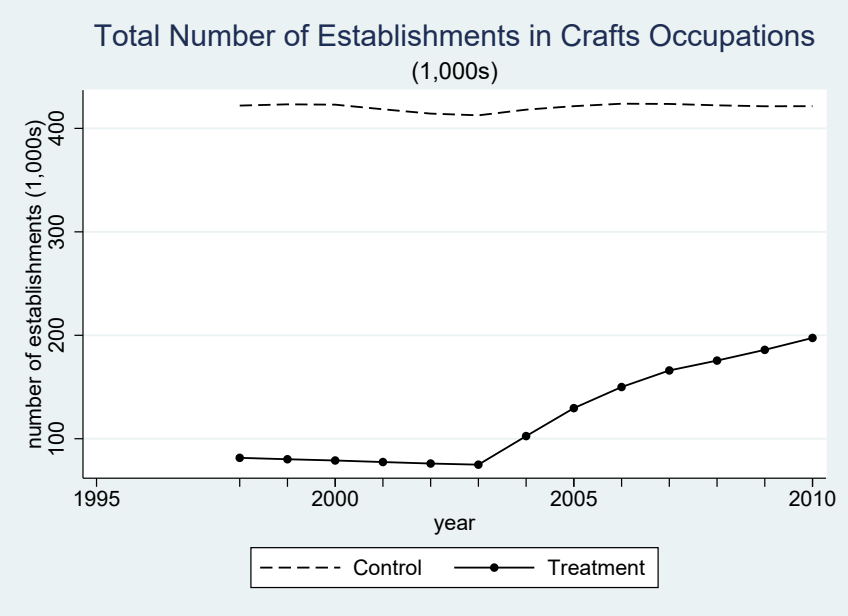
data to analyze the causal effects of regulatory changes pertaining to the product markets of single countries. While these studies have the potential to overcome the above-mentioned concerns, their number is still relatively small.

Our study contributes to this second strand of the empirical literature that evaluates the labor market effects of regulatory changes in the product markets of single countries based on micro data. We consider the 2003 reform of the German Crafts Code to study how the abolishment of barriers to firm entry into a substantial number of the German skilled crafts and trades affected the number of registered crafts businesses, self-employment, as well as dependent employment outcomes in the respective markets. The German system of skilled crafts and trades has deep historical roots and covers a wide range of occupations in areas such as construction, vehicle maintenance, food production, and health services. The skilled crafts cover a significant share of the German workforce. In 2015, there existed about one million registered crafts establishments in Germany (equivalent to about 27.7% of German firms), which employed about 5.36 million employees. This corresponds to about 12.5% of overall employment in Germany (Zentralverband des Deutschen Handwerks (2016)). Until the end of 2003, a total of 93 skilled crafts were subject to the so-called master craftsman requirement, according to which only certified master craftsmen can set up and run a legally independent business in one of the regulated markets. To obtain a master craftsman certificate, individuals have to undergo several years of basic occupational training, collect work experience, participate in preparatory courses, and pass several examinations.[4] Apart from the time investment, there are also course and examination fees which can amount to up to 10,000 Euros in total (Schmidt (2015)). These requirements create a substantial barrier to firm entry into the regulated markets. Accordingly, the German Monopolies Commission has repeatedly criticized the master craftsman requirement as a barrier to firm entry which distorts competition, prices and employment in the German skilled crafts (Monopolkommission (1998, 2002)).[5] In the years leading up to the 2003 reform of the German Crafts Code, it was a widely held view that the German skilled crafts were economically stagnating, and that the regulatory framework was in need of modernization. In March 2003 the German government announced its plans to deregulate firm entry into a large number of crafts occupations as part of the Agenda 2010. In 2004, the master craftsman requirement was fully abolished for 53 out of the 93 initially regulated crafts occupations. Proponents of deregulation of firm entry into crafts occupations argued that the abolishment of barriers to firm entry would boost entrepreneurship, competition, and employment in the German skilled crafts. Indeed, the descriptive evidence is promising. As Figure 4.1 shows based on data provided by the

[4] If individuals want to obtain their master craftsman certificate in an occupation other than their training occupation, they require 3–4 years of occupational experience in the occupation in which they want to obtain the master craftsman certificate. Otherwise, no occupational experience apart from vocational training is required.

[5] As Prantl and Spitz-Oener (2009), p. 772 point out, the master craftsman requirement is not a form of occupational licensing, but rather constitutes a barrier to firm entry into the regulated skilled crafts and trades. In contrast to regulations that govern firm entry, occupational licensing is more general and determines which individuals are allowed to work in a certain occupation.

Figure 4.1: Total Number of Establishments in Crafts Occupations over the Period 1998–2010



Data source: ZDH, own calculations. This graph shows the evolution of the overall number of establishments in the treatment group and the control group in levels (1,000s).

German Confederation of Skilled Crafts (ZDH), at the first glance the reform seems to have been successful in fostering entrepreneurship in the skilled crafts. In 2004, the year in which the reform came into force, the number of registered establishments started rising in the deregulated occupations, and by 2010 it had more than doubled relative to its pre-reform level. Against this background, the 2003 reform of the German Crafts Code is an interesting natural experiment, which, as suggested by Figure 4.1, may have had substantial economic effects.

As we have mentioned above, our paper is related to a relatively small but growing number of studies that provide empirical evidence on the labor market effects of product market deregulation based on micro data.[6] More specifically, one group of papers analyzes the effects of zoning or licensing regulations. In their seminal study on the labor market effects of the "Loi Royer" implemented in the early 1970s in France, Bertrand and Kramarz (2002) provide causal evidence that stricter zoning regulations for large retail stores led to significantly lower retail employment. Their results indicate that this effect was not simply due to a redistribution of employment across sectors. The authors also find that stricter entry regulation in the food retail sector increased food retailer concentration as well as food retailer prices. Changes in market concentration and prices are two specific channels through which entry regulations pertaining to large retail stores may have negatively affected retail employment. Bertrand and

[6] Most of the empirical studies mentioned below use a differences-in-differences approach to evaluate the effects of the respective product market reforms. Bertrand and Kramarz (2002) provide causal evidence based on an instrumental variables approach.

Kramarz (2002) also study the employment effects for certain subgroups of the workforce. Their findings suggest that stricter entry regulation may have favored the employment of male relative to female workers. Furthermore, they show that stricter regulation of entry for large retail stores indeed caused the share of small retail stores to remain relatively higher. Along a similar line, Viviano (2008) studies the effects of entry regulation for large retail stores in Italy. The study provides evidence that the lowering of barriers to entry for large retail stores led to a significant increase in employment in the retail sector, which is mainly driven by employment growth in large retail stores. While there is no significant effect on the share of employment in small retail stores, through the increase in competition the reform negatively affected the share of solo self-employed shopkeepers. In another related paper, Aghion *et al.* (2008) study the economic effects of the abolishment of industrial licensing in the registered manufacturing sector in 1985 and 1991 in India. Specifically, the authors analyze how the effects of product market deregulation interact with labor market regulations that vary across Indian states. They find that the abolishment of industrial licensing led to a significant increase in the number of factories in the respective industries, and that the effects of product market deregulation were mitigated by the state-specific strictness of labor market regulations. Both real output, total employment and fixed capital in the respective industries grew more strongly in states with pro-employer labor market regulations relative to states with more protective labor market regulations.

Other related papers study the effects of product market reforms that directly reduced the extent of procedures and, thus, the time and expenditures required for creating business start-ups. Several studies evaluate the labor market effects of product market deregulation in Mexico (Bruhn (2011, 2013); Kaplan *et al.* (2011)). Starting in 2002, Mexican municipalities began to strongly simplify and speed up local business registration procedures. The introduction of the so-called "Rapid Business Opening System" led to a statistically significant increase in firm entries (Bruhn (2011); Kaplan *et al.* (2011)) – although the studies differ in their assessment of the economic significance of the effect. The empirical evidence also indicates a positive effect on employment, but the analysis in Bruhn (2011) shows that this was mainly due to a redistribution of workers, so overall employment was not affected. The lowering of barriers to firm entry also led to a decrease in prices (Bruhn (2011)). Bruhn (2013) finds that the lowering of barriers to firm entry also affected the behavior of informal business owners in Mexico. For informal business owners with characteristics similar to those of wage workers the reform increased the probability to leave self-employment and become wage workers. For a subgroup of informal business owners with characteristics similar to those of official business owners, the author finds an increase in the probability to officially register the existing informal business. Branstetter *et al.* (2013) study the effects of the "On the post firm" program, which was introduced in Portugal in 2005 and substantially simplified and sped up the registration of business start-ups. They find that the reform had statistically significant positive effects on firm entry and job creation in new firms, but that the economic significance of the effects was relatively small due to the nature of the newly

created firms. Branstetter *et al.* (2013) find that the reform mainly led to the creation of so-called "marginal firms". The respective firms are small and have lower survival rates. In addition, the new entrepreneurs are "marginal" in the sense that they are typically former dependent employees. They are also older and less well educated than the comparison group.

A more distantly related paper is Bertrand *et al.* (2007), which evaluates the 1985 deregulation of the French banking system. The authors show that along with reform-induced changes in the capital structure and banks' lending behavior, the reform also had an impact on real economic outcomes. The authors find that the reform led to increased firm turnover (through higher entry of new firms, and higher exit of incumbent firms) and lower industry concentration. At the same time, the reform also led to a statistically significant increase in employment and lower wage growth at the industry level.

Of course, we are not the first to study the economic consequences of the regulatory framework which applies to the skilled crafts and trades in Germany. Prantl and Spitz-Oener (2009) exploit the German reunification in 1990 as a natural experiment to study the differential effects of the regulatory barriers imposed by the German Crafts Code between two very different economic contexts: that of the mature West German economy, and that of the transitional East German economy. The analysis covers the 1980s and 1990s, that is the period leading up to the 2003 reform of the German Crafts Code. The authors find that entry regulation hampered self-employment and the occupational mobility of workers after reunification more in regulated occupations in East Germany than in regulated occupations in West Germany. Thus, in the economic environment of East Germany after reunification, where high degrees of flexibility were required from all labor market participants to facilitate economic transition, the barriers to market entry created by the German Crafts Code proved to be especially limiting. Prantl (2012) uses a similar identification strategy to analyze the effects of firm entry regulation on sustained entry into self-employment, as well as on the performance of long-living entrants based on data covering the 1990s. After reunification, due to firm entry regulations the probability of sustained self-employment with a duration of at least five years was significantly lower in East than in West Germany. This effect was not compensated by better performance of newly created long-living firms, which leads the author to conclude that barriers to firm entry may have negative long-term effects on outcomes such as economic growth and technological progress.

Based on individual-level survey data from the Microcensus wave of 2006, Bol (2014) analyzes whether regulation of entry into self-employment in crafts occupations is associated with economic rents. The author compares the wage differential between self-employed and dependently employed craftsmen in regulated relative to deregulated crafts occupations. As the analysis only uses the survey wave 2006, it does not evaluate the effects of the 2003 reform of the German crafts code, but it exploits the regulatory differences created by the 2003 reform. The linear regressions include occupation-level controls that capture the demographic composition and occupation-specific skill requirements obtained from the BIBB-BAuA Workforce survey. Bol (2014) finds that overall employment in the regulated

occupations is not associated with higher wages relative to the deregulated occupations, but that self-employed craftsmen in regulated occupations earn about 13% higher wages than those in deregulated occupations. According to the author, this suggests that the barriers to entry imposed by the German Crafts Code indeed create economic rents for incumbent self-employed craftsmen. Against this background, Damelang *et al.* (2016) study the effects of the 2003 reform of the German Crafts Code on wages of prime-aged full-time employees in the crafts occupations based on the administrative data set SIAB for the period 2000–2008. They argue that the deregulation of entry into crafts occupations led to increased competition and, thus, to a lowering of economic rents. In turn, this should lead to lower average wages in the deregulated crafts occupations. Using a differences-in-differences design, Damelang *et al.* (2016) find that deregulation had a negative effect on wage growth, with post-reform wages in deregulated occupations growing about 0.5% less than those in regulated occupations. The effect was more pronounced for unskilled employees relative to skilled employees, while highly skilled master craftsmen realized relatively higher wage growth. For young workers the reform was wage-neutral, and the overall negative wage effects were driven by older workers. The authors argue that this effect heterogeneity is due to differences in bargaining power. Younger or more highly skilled employees have higher bargaining power due to higher fluctuation costs incurred by the employer as well as due to the existence of a credible outside option: they could leave their current employer and found their own business start-up.

In addition to this, Rostam-Afschar (2014) studies how the 2003 reform of the German Crafts Code affected self-employment based on individual-level survey data from the Microcensus for the period 2002–2009. The author applies a differences-in-differences approach with one control and three treatment groups. Apart from the set of fully deregulated occupations, the author analyzes two additional treatment groups, which remained subject to the entry regulations, but for which possible exemptions from the master craftsman requirement were introduced. As a control group, Rostam-Afschar (2014) only considers the small subgroup of continuously regulated occupations for which hardly any regulatory changes occurred through the 2003 reform of the German Crafts Code. The author studies how the reform affected the probability of being self-employed, as well as entry and exit probabilities. For the group of fully deregulated crafts occupations, Rostam-Afschar (2014) estimates that the abolishment of barriers to entry led to an increase in the probability of being self-employed by about 40%. For the respective treatment group, this corresponds to an increase in the probability of entry into self-employment by about 60%, while the author finds that exit rates were unaffected by the reform. The other two treatment groups exhibit qualitatively similar, but quantitatively less pronounced results. As part of the robustness checks, the author shows that his results are neither confounded by the EU expansion to the East nor are they confounded by the the introduction of start-up subsidies. Furthermore, the author finds that the positive reform effect on entry probabilities was driven by male craftsmen and by untrained workers. However, given that he finds no reform effect on exit rates, the author nevertheless states that “more sustainable business entries could

be established after the deregulation" (Rostam-Afschar, 2014, p. 1088). In a very recent paper, Runst *et al.* (2016) conduct a replication study of Rostam-Afschar (2014). Using a different approach to identifying crafts occupations in the Microcensus data, and based on a differences-in-differences design with two instead of three treatment groups, the findings in Runst *et al.* (2016) basically corroborate the evidence of a positive reform effect on the probability of being self-employed and on the probability of entry into self-employment.[7] In contrast to Rostam-Afschar (2014), the authors find that the reform also significantly increased the probability of exit from self-employment in the deregulated occupations by about 1.5 percentage points. This effect on exit probabilities emerges if Runst *et al.* (2016) do not control for educational attainment in the differences-in-differences estimations. The authors therefore conclude that the increase in exit probabilities is mainly due to the relatively lower educational attainment of new entrepreneurs. This is also in line with Runst *et al.* (2016)'s observation that, according to firm registry data provided by the German Confederation of Skilled Crafts (ZDH), in the deregulated crafts occupations average measures of educational attainment of entrepreneurs have deteriorated relative to the pre-reform period.

Our study exploits the 2003 reform of the German Crafts Code as a natural experiment to study the effects of product market deregulation on labor market outcomes. We implement a differences-in-differences approach using data sets provided by three different sources: data provided by the German Confederation of Skilled Crafts, survey data provided by the Federal Statistical Office of Germany (Microcensus), as well as administrative data provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (SIAB). Our study aims at analyzing how the abolishment of barriers to firm entry into a substantial number of the German skilled crafts affected the number of registered crafts businesses, self-employment, as well as dependent employment outcomes in the respective markets. Regarding the focus on employment outcomes, our study is closely related to Rostam-Afschar (2014) and Runst *et al.* (2016). However, it is important to notice that our aim was never to perform a replication of the former study. Our empirical design fundamentally differs from that of Rostam-Afschar (2014) and Runst *et al.* (2016) along several important dimensions. Most importantly, we only consider one treatment group instead of three (or two, as in the case of Runst *et al.* (2016)), and we choose different sets of treatment and control occupations. Regarding the empirical design, our study is thus much more similar to the approach taken by Damelang *et al.* (2016), although they focus on other outcome variables. In addition, we expand on the analysis conducted in Rostam-Afschar (2014) and Runst *et al.* (2016) by providing differences-in-differences results on dependent employment outcomes. The above-cited related literature mostly focuses on the effects of

[7] Runst *et al.* (2016) report that according to firm registry data, the treatment occupation *building cleaners* underwent a singularly positive development during the post-reform period, which is not reflected in the Microcensus data. Since their research indicates that this specific crafts occupation might not be properly identified in the Microcensus data, they exclude it from the main estimation samples. In contrast to Runst *et al.* (2016), our estimation samples contain the occupation *building cleaners*. However, our robustness checks, which involve sequentially dropping single occupations from the estimation samples, indicate that our results are not driven by this specific treatment occupation.

product market deregulation on self-employment, and the papers typically only provide limited results on dependent employment effects. However, as the theoretical literature indicates, the effects of product market deregulation on dependent employment could be substantial. We therefore provide a detailed regression analysis of dependent employment outcomes.

Until the reform came into force in the beginning of 2004, all crafts occupations in the treatment and the control group were subject to the same regulatory framework that strictly regulated firm entry into the respective skilled crafts through the master craftsman requirement. This initially led us to expect that treatment and control occupations should be fairly comparable. We therefore use a differences-in-differences design to study the labor market effects of the 2003 reform of the German Crafts Code. Unfortunately, a closer investigation of the institutional background and extensive empirical checks cast doubts on the validity of the identifying assumptions. This leads us to conclude that the assumption of common trends is not fully plausible, and that there are significant differences not only in the levels, but also in the pre-reform evolution of some of the outcome variables. The view that regulated and deregulated crafts occupations are not sufficiently comparable is also in line with a very recent paper by Müller (2016). We therefore cannot view our estimation results as reflecting the causal effects of product market regulation on labor market outcomes. This paper therefore mainly documents our analysis of the reform, and we are very careful with the overall interpretation of our results. We find that after the reform, the number of registered crafts establishments as well as the number of self-employed craftsmen increased more strongly in the deregulated crafts occupations relative to the regulated ones. The opposite holds for the number of dependently employed craftsmen. Individual-level regressions suggest a positive effect on the probability of being newly self-employed among all self-employed, and negative effects on dependent employment probabilities. While we cannot interpret the empirical results causally, the analysis at least partially corroborates the evidence for a positive reform effect on entrepreneurship and self-employment documented elsewhere in the literature (Rostam-Afschar (2014); Runst *et al.* (2016)), while the reform seems not to have had a positive effect on dependent employment in the deregulated markets.

These conclusions are also in line with Müller (2016), who in a very recent study assesses the economic effects of the 2003 reform of the German Crafts Code based on a wide range of descriptive statistics that reach as far as the year 2014. The author provides graphical evidence and descriptive statistics which suggest that the abolishment of barriers to firm entry indeed fostered entrepreneurship in the deregulated markets. However, he also reports that the newly founded businesses in the deregulated occupations tended to be relatively small and had lower survival rates. The share of solo-entrepreneur businesses reached about 61% in the deregulated occupations in 2012, and the five-year firm survival rates in the deregulated occupations dropped from 69% in early 2000s to below 50% in the post-reform period. Since the reform, the number of master craftsman examinations as well as apprenticeship training rates decreased more strongly in the deregulated occupations relative

to the regulated occupations. Furthermore, graphical evidence in Müller (2016) suggests that until 2012 dependent employment did not increase in the deregulated occupations relative to the regulated ones.[8] Additionally, the author reports that a substantial share of employment growth (about 60%) in the deregulated occupations occurred through growth in part-time employment. Regarding the effects of the reform on entrepreneurship, employment, prices, and human capital creation in the German skilled crafts and trades, the author concludes that "the proponents' high hopes were not fulfilled" (Müller, 2016, p. 21).

The remainder of this paper is structured as follows: Section 4.2 describes the institutional background. Section 4.3 describes the empirical approach, the data and sampling rules. It also discusses the plausibility of the common trends assumption and potential confounding factors. In Section 4.4 we present and discuss our main empirical results. Section 4.5 concludes. The Appendix 5.3 contains additional tables and figures (Tables and Figures starting with "A.") and further background information, such as detailed information on the variables and samples used in our analysis, as well as summaries of test results regarding the plausibility of the common trends assumption.

4.2 Institutional Background

There is a long tradition of strict regulation of firm entry into the skilled crafts and trades in Germany. After World War II, in 1953 German parliament passed a new Crafts Code ("Handwerksordnung") that continued to support strong entry regulations. According to this law, only master craftsmen were allowed to start and operate a legally independent crafts enterprise (principle of "Großer Befähigungsnachweis"). In a reform of the crafts law in 1998, the number of registered crafts occupations was reduced by grouping related occupations to allow crafts enterprises to offer a broader range of services. However, the requirement of a master craftsman certificate for starting and operating a crafts enterprise remained in place (Brenke (2008)).

In the late 1990s the German Monopolies Commission ("Monopolkommission") criticized the barriers to entry created by the German Crafts Code and the resulting negative economic consequences such as a high price level, negative employment effects, and increased black labor ((Monopolkommission, 1998, pp. 16—17)). It repeatedly recommended the abolishment of the master craftsman requirement (Monopolkommission (1998), Monopolkommission (2002)). At the same time, the process of European integration put further pressure on German politicians to modernize the German Crafts Code (see Brenke (2008) for further details). Against this background, as part of the Agenda 2010 reforms, in March 2003 the

[8] According to Figure 5 Müller (2016), p. 11, dependent employment grew relatively more strongly in the deregulated occupations over the period 2010–2012 if one includes the deregulated occupation *building cleaners* in the sample. However, the author argues that the economic performance of this specific crafts occupation was exceptional and very likely driven by factors independent of the 2003 reform of the German Crafts Code. If one excludes the occupation *building cleaners* from the sample, the observed differences during the period 2010–2012 vanish.

German government announced its plans for a major reform of the German Crafts Code with the aim of boosting entrepreneurship in crafts occupations (Müller (2006)). The first draft of the bill was presented to the German Bundestag on 24. June 2003. It contained, among others, the following major alteration to the German Crafts Code (Deutscher Bundestag (2003b)): The number of crafts occupations subject to the master craftsman requirement (listed in appendix A of the Crafts Code) was to be reduced from 94 to 29 occupations. Only risky ("gefahrengeneigt") crafts occupations, that were related to potentially serious health and safety risks for customers, should remain subject to the master craftsman requirement.

In 2003 all major political parties agreed on the need for modernizing the German Crafts Code to create a regulatory framework that would allow for a more dynamic development of entrepreneurship in the crafts occupations. However, there was dissent on the exact nature of the required reforms (Deutscher Bundestag (2003a,c,d)). The government saw the master craftsman requirement as a significant barrier to firm entry into crafts occupations and as a significant impairment of competition. Accordingly, the main argument put forward by the proponents of the reform was that the abolishment of barriers to firm entry would promote competition among enterprises in the respective craft occupations, which would lead to lower prices, increased market entry and higher employment. Only crafts occupations that were related to potentially serious health and safety risks for customers should remain subject to the master craftsman requirement. However, craftsmen working in deregulated occupations may still choose to earn a master craftsman certificate to signal a level of high occupational qualification, which should then be rewarded through the market mechanism.

The German Confederation of Skilled Crafts (ZDH) and the political opposition strongly opposed the abolishment of the master craftsman requirement based on several arguments. Firstly, they argued that rather than being a barrier to entry, the master craftsman degree should be seen as an educational attainment crucial to running a crafts business successfully. They argued that the deregulation of crafts occupations would lead to a decrease in service and product quality, a decrease in customer safety and higher bankruptcy rates among newly created crafts enterprises. Secondly, deregulation would impair incumbents' economic performance through increased competition and would, thus, negatively affect employment in the skilled crafts. Furthermore, crafts enterprises would no longer have an incentive to train apprentices, leading to a strong decrease in the availability of apprenticeship training positions and an increase in youth unemployment. Based on these arguments, the ZDH and the political opposition demanded that the criteria for the choice of deregulated occupations should be revised. Apart from potential health and safety risks for customers, most importantly, one should also take into account the extent of apprenticeship training provided by the respective crafts occupations.

Despite the above-mentioned controversies, the bill was passed by the German Bundestag on 27. November 2003, and then, according to legislative procedure, was presented to the Federal Council of Germany. However, due to opposition by some of the federal states represented in the Federal Council, the proposed bill was directed towards the Mediation

Committee ("Vermittlungsausschuss") (Bundesrat (2003)), and a revised version of the bill was passed both by the German Bundestag and by the Federal Council on 19. December 2003.[9] As a result of the mediation process, in the final version of the law designed to reform the German Crafts Code, the extent of apprenticeship training provided in a crafts occupation was adopted as an additional criterion to identify those crafts occupations that should remain subject to the master craftsman requirement. Correspondingly, the number of regulated crafts occupations, which were listed in Appendix A of the Crafts Code, was only reduced from 94 to 41 – instead of an initially planned 29 occupations. For 53 crafts occupations the master craftsman requirement was abolished and they were moved to Appendix B1 of the Crafts Code.[10] The reform came into force shortly afterwards on 1. January 2004.

4.3 Empirical Approach

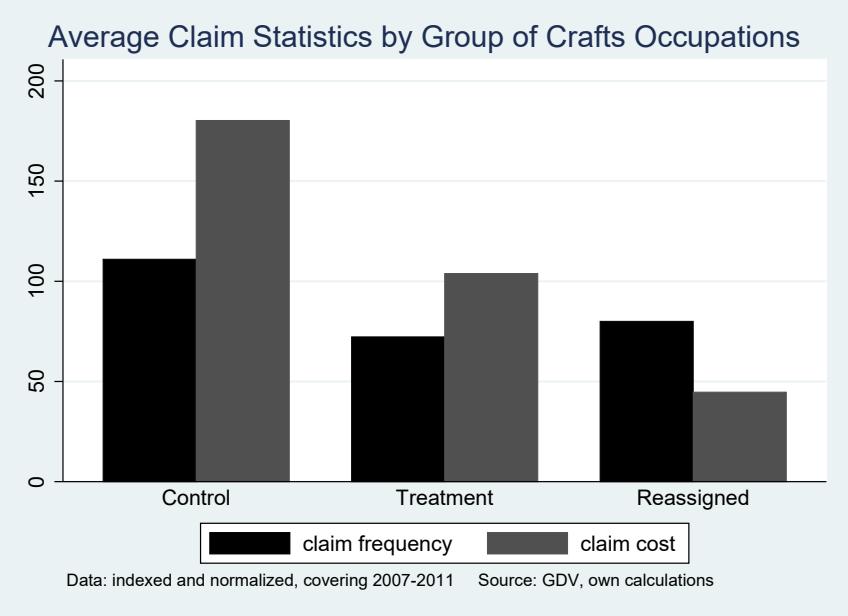
The objective of this paper is to study how product market deregulation affects labor market outcomes. We exploit the 2003 reform of the German Crafts Code as a natural experiment to study the interdependencies between product market deregulation and labor market outcomes based on a differences-in-differences (DiD) design. In our analysis, we compare deregulated (treatment) to non-deregulated (control) crafts occupations. Ideally, assignment into the two groups should be random. However, given the background information in Section 4.2, we cannot assume that assignment of crafts occupations into treatment was fully random. Initially, assignment into treatment followed the relatively objective criterion of whether a crafts occupation was related to potentially serious health and safety risks for customers. Most importantly, it is plausible that this characteristic is not systematically related to the profitability of doing business in the respective markets. The political debate between proponents and opponents of the reform and, ultimately, the alterations proposed by the Mediation Committee then led to the implementation of a second choice criterion: the extent of apprenticeship training provided by the skilled crafts. Consequently, instead of the initially planned 65 crafts occupations, firm entry into only 53 occupations was fully deregulated. The fact, that through political negotiations 12 crafts occupations were reassigned from becoming deregulated to staying regulated, leads us to assume that the respective occupations were special. For example, this may have been the occupations where fighting for upholding barriers to entry was most economically rewarding for incumbents.

As an illustration to the first choice criterion mentioned above, Figure 4.2 provides a rough overview of two indicators from insurance claim statistics for the period 2007–2011 provided to us by courtesy of the German Insurance Association (GDV). The data refers to the post-

[9] For a comprehensive overview of the major steps of the legislative process pertaining to this specific law see Deutscher Bundestag (2016).

[10] Compare "Drittes Gesetzt zur Änderung der Handwerksordnung und anderer handwerksrechtlicher Vorschriften" in its version of 24. December 2003 (Bundesgesetzblatt, Teil I 2003, Nr. 66, 29.12.2003, pp. 2934–2953).

Figure 4.2: Average Insurance Claim Statistics by Group of Crafts Occupations Covering the Period 2007–2011



Source: data provided by courtesy of the German Insurance Association (GDV), own calculations. Claim frequency is the number of claims divided by the total number of employees insured. Claim cost relates cost to risk exposure, i.e. it is the average cost (payments made by the insurance companies) per employee insured. The underlying indices are based on a comparison of similarly sized establishments. Indices are normalized – a value of 100 corresponds to the outcome average across all crafts occupations. Graph based on a subset of 5 control, 12 treatment and 4 reassigned occupations.

Table 4.1: Descriptive Statistics for Three Groups of Crafts Occupations. Occupation-level Averages Over the Pre-reform Period 2000–2003

	Treatment	Control	Reassigned
Number of occupations:	53	29	12
Number of establishments:			
mean	1450.3	14382.5	14768.6
standard deviation	(2348.1)	(18762.4)	(19080)
min	33	116	292
max	12401	64146	65975
Apprenticeship graduates per establishment:			
mean	8.1	17.5	15.5
standard deviation	(6.1)	(8.7)	(11.2)
min	0	1.2	2.3
max	28.3	34.5	43.8
Number of master craftsman exams (including retakes):			
mean	42.5	666.2	492.1
standard deviation	(89.7)	(939.8)	(722.1)
min	0	0	3
max	570	3759	2590

Data Source: ZDH, own calculations.

reform period and only covers a subset of about 20 crafts occupations.[11] Nevertheless, the resulting picture is in line with what we would expect given the institutional background information: Firstly, on average control occupations indeed exhibit a higher claim frequency and claim cost than treatment and reassigned occupations. Secondly, the group of reassigned crafts occupations appears to be more similar to the treatment occupations than to the control occupations both with respect to claim frequency as well as with respect to claim cost.[12] Although these graphical results are only based on a small sub-sample of the relevant crafts occupations, and thus should be treated with caution, they suggest that the risk-related choice criterion was properly applied.

Furthermore, Table 4.1 provides descriptives on the three different groups of crafts occupations for the pre-reform period 2000–2003. Interestingly, the group of reassigned occupations is similar to the group of control occupations with respect to the average number of apprenticeship graduates per establishment. However, if we compare minima and maxima across groups, it is apparent that some occupations in the treatment group actually produce more apprenticeship graduates than the less active occupations in the reassigned group. Therefore, the criterion relating to the provision of apprenticeship training seems to have been applied

[11] Identification of crafts occupations is very difficult in the insurance claims data due to the aggregation of crafts occupations at the time of data collection. We were only able to identify approximately 5 control, 12 treatment and 4 reassigned occupations, and some groupings include non-crafts occupations.

[12] We are very grateful to the German Insurance Association (GDV), especially to Dr. Marco Lonsing, for providing us with data on insurance claims in the German skilled crafts.

in a somewhat arbitrary manner.

While we cannot assume random assignment of crafts occupations into treatment, being able to observe the reassignment of crafts occupations through the legislative process at least allows us to isolate the respective occupations. Therefore, in our differences-in-differences analysis we consider as treatment occupations all 29 occupations that were always (starting with the draft bill) chosen to be deregulated based on the criterion of being risky. The control group consists of those 53 crafts occupations that were initially assigned to becoming deregulated, and remained so in the amended version of the bill. However, from our analysis we fully exclude those 12 crafts occupations that were reassigned from becoming deregulated to staying regulated. They are not part of the control group. As the list of crafts occupations provided in Table A.31 shows, the treatment and the control group now contain a number of seemingly closely related occupation pairs. For example, the treatment group includes *optics technicians and precision opticians*, while *opticians* are included in the control group. Likewise, *turners* are included in the treatment group, while *metalworkers* are included in the control group.

Based on this definition of treatment and control occupations, we estimate the average effect of the deregulation of firm entry in the treatment occupations on labor market outcomes such as the registered number of crafts businesses, self-employment, and dependent employment. For this, we compare the difference in average outcomes between the treatment and the control group after the reform with the corresponding difference before the reform. Our analysis which uses individual-level data is based on the following base specification:

$$Y_{iot} = \alpha + \gamma_t + \delta_o + \beta_3 \cdot PR_t \cdot TG_o + X_{it}'\varepsilon + u_{iot},$$

where Y_{iot} is the outcome of interest, which is a dummy variable. Index i refers to individuals, o indexes occupations and t indexes years. α is the model constant. γ_t are year fixed effects which we include in our model to control for systematic differences across survey waves. Likewise, we include occupation fixed effects δ_o to control for any systematic variation in the occupational composition of our treatment and our control group. Most importantly, β_3 is the coefficient of interest, with PR_t representing a dummy indicating the post-reform period and TG_o representing a dummy indicating treatment occupations. The vector X_{it} then covers a number of individual-level socio-demographic covariates, with ε being the corresponding vector of regression coefficients.

In line with most of the related literature we use ordinary least squares (OLS) estimation to implement our differences-in-differences approach, even in the case of binary outcome variables. The estimation of linear probability models is appropriate in differences-in-differences settings since most regressors are discrete and, therefore, fitted probabilities that lie outside the 0–1 range are less of a concern (Angrist and Pischke (2009), pp. 102–107; Wooldridge (2010), pp. 562–565). However, as a robustness check we also estimate

corresponding non-linear Probit models for the most basic specifications without wave and occupation fixed effects, and we then calculate average marginal effects for the interaction term of interest $(PR_t \cdot TG_o)$ (Ai and Norton (2003); Norton *et al.* (2004)). A comparison reveals that the respective estimation results do not depend on the choice of the estimation approach.

Throughout the analysis, in all differences-in-differences estimations based on individual-level data we use standard errors clustered at the occupational level as recommended by Bertrand *et al.* (2004), pp. 270–272 to deal with serial correlation in settings with a number of clusters $N \geq 50$. The inclusion of occupation fixed effects in the presence of clustered standard errors presents us with a problem, since in the respective specifications the number of parameters to be estimated exceeds the number of clusters. This may lead to unreliably estimated standard errors. Therefore, in all individual-level regressions labeled "with occupation fixed effects" we actually perform a within-occupation transformation of the data. We calculate means of all variables by occupation and deduct these means from both the respective dependent and the respective independent variables. We then run the full set of regressions on the de-meaned variables. The resulting point estimates are identical to those in the model with dummy-occupation fixed effects, but since we do not need to explicitly estimate the occupation fixed effects any more, we now obtain the correct standard errors and the F-statistics.

In cases where individual-level data is not available, or when we explicitly want to study aggregate outcomes at the occupational level, we run differences-in-differences estimations on data aggregated at the occupation-year level. Based on the occupations panel, we use OLS estimation with heteroscedasticity-robust standard errors to estimate the following differences-in-differences base specification:

$$Y_{ot} = \alpha + \gamma_t + \delta_o + \beta_3 \cdot PR_t \cdot TG_o + u_{ot},$$

where Y_{ot} is the outcome of interest in levels. Instead of Y_{ot}, in some specifications we use conventionally calculated growth rates $\left(\frac{Y_{opost} - Y_{opre}}{Y_{opre}}\right)$ as the outcome variable. α is the model constant. γ_t are year fixed effects and δ_o are the occupation fixed effects δ_o. As explained above, β_3 is the coefficient of interest, with PR_t representing a dummy indicating the post-reform period and TG_o representing a dummy indicating treatment occupations. We do not include other occupation-level covariates in the specifications. Thereby, we assume that over the period of observation all crafts occupations should be affected similarly by macroeconomic events.

Another solution proposed by Bertrand *et al.* (2004), pp. 267–269 to the problem of serial correlation, which is typically present in differences-in-differences designs, is the aggregation of panel data covering multiple years into a simple two-period panel. As a further check on our occupation-level results, we therefore collapse the occupations panel

into a two-period panel of crafts occupations by taking the averages of all variables over the pre-reform years (2000–2003) and the post-reform years (2005–2008), respectively. We then calculate conventional growth rates to assess the pre-2004 versus post-2004 changes in all relevant outcome variables. Finally, we estimate the following first-differences (FD) model:

$$\left(\frac{Y_{opost} - Y_{opre}}{Y_{opre}}\right) = \alpha + \beta_2 \cdot TG_o + u_o,$$

where Y_{opre} is the average outcome of interest over the pre-reform period and Y_{opost} is the average outcome of interest over the post-reform period. α is the model constant. β_2 is the coefficient of interest, with TG_o representing a dummy indicating treatment occupations. We use OLS with heteroscedasticity-robust standard errors to estimate the above-described model.

4.3.1 Plausibility of the Common Trends Assumption

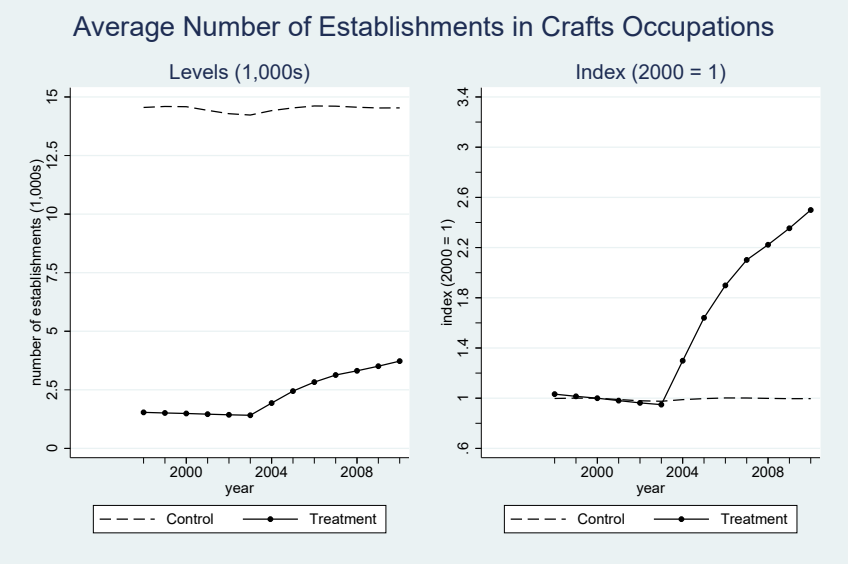
The differences-in-differences approach chosen in this study relies on the common trends assumption. We must assume that the outcomes of interest would have evolved along similar trends in the treatment and control occupations had the 2003 reform of the German Crafts Code not happened. Since the nature of this argument is counterfactual, we cannot explicitly check whether the assumption holds. In this section we therefore provide graphical evidence and discuss test results to investigate whether the outcomes of interest in the two groups of crafts occupations evolved similarly over the pre-reform period. If we observed that the two groups of crafts occupations evolved similarly until 2003, then it would be plausible that they could also have evolved similarly afterwards.

As Figure 4.1, p. 72 has already shown, until 2003 the evolution of the total number of registered establishments was parallel in the treatment and the control occupations. Figure 4.3, p. 86 adds to this by plotting the average number of registered establishments both in levels as well as in the form of an index with base year 2000. The index in the right panel of Figure 4.3 shows that the evolution of the outcome of interest was very similar between both groups during the pre-reform period. Figure 4.4, p. 87 shows the evolution of the average number of self-employed in crafts occupations. Again, we observe that although the two groups differ in terms of levels, the outcome of interest evolved similarly over the pre-reform period, and then diverged starting in 2005.

Figure 4.5, p. 87 shows the evolution of dependent employment as reflected by full-time equivalents in the skilled crafts based on survey data (Microcensus). In both groups, relative changes in full-time equivalents, as reflected by the indices, followed a negative trend before the reform, but the trends were not fully parallel.[13] Furthermore, full-time equivalents did

[13] Rostam-Afschar (2014), p. 1071 argues that the years before 2002 could still be influenced by the aftermath of previous changes in the regulatory regime and, therefore, should be excluded from the analysis. However, we

Figure 4.3: Average Number of Establishments in Crafts Occupations over the Period 1998–2010



Data source: ZDH, own calculations. This graph shows the evolution of the average number of establishments per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2000 (right panel).

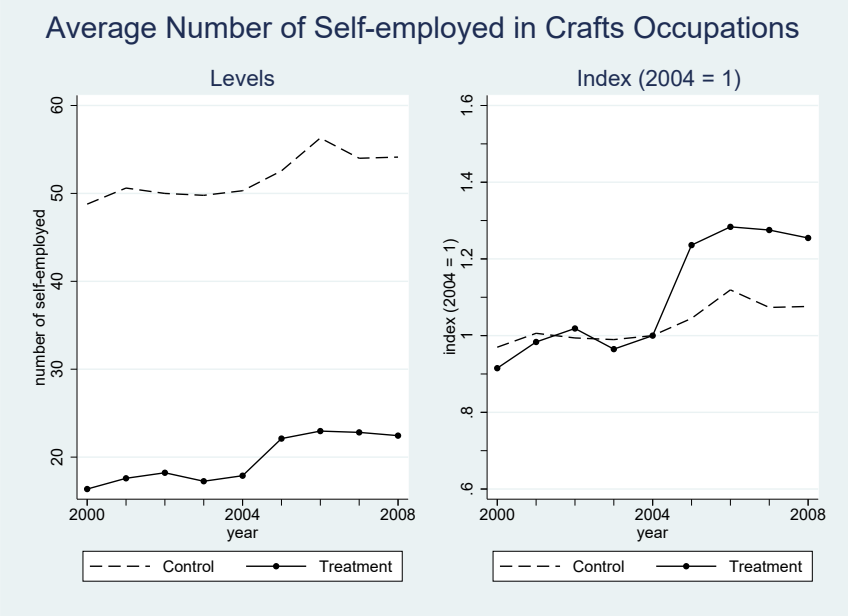
not evolve along parallel trends in absolute terms. A similar picture emerges when we plot the evolution of the average number of dependently employed in Figure A.36. However, the pre-reform trends in the indices look more parallel if we differentiate between full-time and part-time employment. We observe that over the pre-reform period the average number of full-time employed (compare Figure A.37) as well as the average number of part-time employed (compare Figure A.38) evolved fairly similarly in relative terms, but not in absolute terms.

Figure 4.6, p. 88 shows the evolution of dependent employment in the form of full-time equivalents based on administrative data (SIAB). Full-time equivalents evolved similarly in the two groups in relative terms but not in absolute terms. In addition to this, Figures A.39–A.41 provide graphical evidence on the evolution of the average number of dependently employed craftsmen in total, as well as separately for full-time and part-time employed craftsmen. Again, a comparison of treatment and control occupations reveals that total dependent employment and full-time employment evolved fairly similarly over the pre-reform period in relative terms, but not in absolute terms. Furthermore, Figure A.41 indicates that over the pre-reform period the average number of part-time employed evolved differently in the two groups of crafts occupations. It decreased in the treatment occupations, while it was rather volatile in the control occupations.

In addition to the graphical results provided above, we also plot the evolution of all

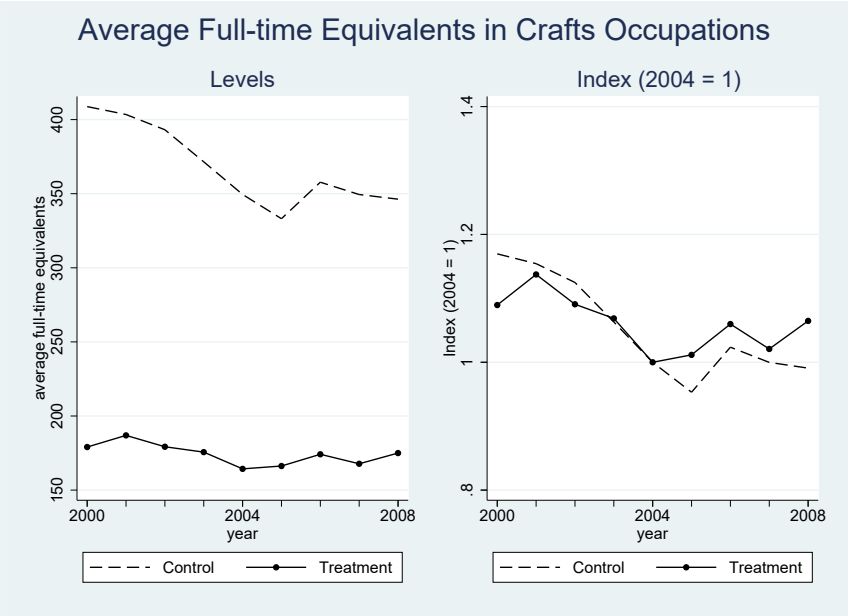
view this time cutoff as somewhat arbitrary, and we want to keep the number of sampled years in the pre- and post-reform period symmetric.

Figure 4.4: Average Number of Self-employed in Crafts Occupations over the Period 2000–2008



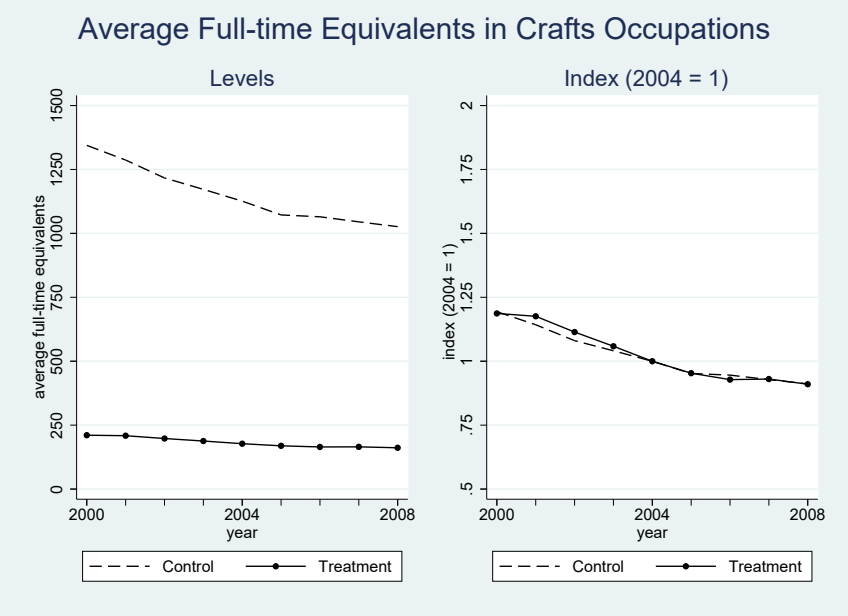
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average number of self-employed per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure 4.5: Average Full-time Equivalents in Crafts Occupations over the Period 2000–2008 Based on Survey Data



Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of average full-time equivalents per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure 4.6: Average Full-time Equivalents in Crafts Occupations over the Period 2000–2008 Based on Administrative Data



Data source: SIAB 2000–2008, own calculations. This graph shows the evolution of average full-time equivalents per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

outcomes of interest used in the individual-level regressions. Furthermore, we perform statistical tests to investigate whether the outcomes evolved along similar trends in treatment and control occupations over the pre-reform period 2000–2003. Further graphs, an explanation of the test specifications, and a detailed summary of the respective test results can be found in Appendix 5.3.3. The graphical checks and trend estimations suggest that the common trends assumption is plausible for most but not for all of the outcomes of interest. For three individual-level outcomes we find statistically significant differences in pre-reform trends between treatment and control occupations.[14]

A further way to investigate the plausibility of the common trends assumption is to test for the effects of placebo reforms. We implement such tests where we treat the data as if the reform had already come into force in the beginning of 2002 or in the beginning of 2003, respectively. Ideally, such placebo regressions should yield statistically insignificant estimates that are close to zero in absolute size. Appendix 5.3.4 contains an explanation of the placebo tests and a detailed discussion of the test results. Unfortunately, the placebo tests corroborate the evidence that the two groups of crafts occupations did not evolve similarly during the pre-reform period. Especially for the dependent employment outcomes

[14] In light of the discussion about whether or not to include the years 2000–2001 in the estimation sample, it is worth mentioning that once we drop the year 2000 from the sample, only in the case of the probability of entry into dependent employment we still find statistically significantly different pre-reform trends. At the same time, the respective estimated effects of interest in the differences-in-differences estimations are robust to dropping the year 2000 from the sample.

the placebo tests yield problematic estimates. The same holds for the first-differences placebo results on growth in the the number of self-employed craftsmen.

Overall, the graphical results and the test regressions indicate that over the pre-reform period some labor market outcomes evolved differently across treatment and control occupations. Most importantly, the placebo tests indicate that there was considerable pre-reform heterogeneity in the evolution of dependent employment outcomes across treatment and control occupations. This indicates that we have to view our estimation results with caution. We cannot interpret the regression results as reflecting the causal effects of the 2003 reform of the German Crafts Code. We will come back to this in the consolidating discussion of the estimation results in Section 4.4.5.

4.3.2 Potentially Confounding Factors

A common concern in differences-in-differences scenarios is that economic agents may expect the upcoming regulatory change and may adapt their economic behavior accordingly well in advance. This would then bias the estimated effects. Fortunately, the relatively short time span of the legislative process (the reform plans were announced in March 2003 and the law already came into force on 1. January 2004), and the considerable uncertainty created by the political debate over choice criteria and the exact choice of deregulated occupations, make such adaptive behavior very unlikely. Most importantly, one has to keep in mind that the Mediation Committee did not present its recommended alterations to the bill to the German Bundestag until 16. December 2003, only about two weeks before the law finally came into force. Thus, pre-reform adaptive behavior is most likely not a concern in the context of this reform.

Furthermore, one also has to consider how the 2003 reform of the German Crafts Code affected entry regulation in the control occupations that remained subject to the master craftsman requirement after 1. January 2004. Apart from the complete abolishment of the master craftsman requirement in the treatment occupations, the reform also partially reduced barriers to firm entry into the control occupations. Firstly, the abolishment of the so-called “Inhaberprinzip” implied that as of 1. January 2004 the owner of a crafts enterprise operating in one of the regulated markets was no longer required to hold a master craftsman certificate personally as long as he hired an operations manager who held a master craftsman certificate.[15]

Secondly, the reform strengthened special exemptions from the master craftsman requirement for journeymen with long work experience. For this specific group of craftsmen, special exemptions from the master craftsman requirement already existed before 1. January 2004, but the 2003 reform of the German Crafts Code was meant to further facilitate the application of this so-called “Altgesellenregelung”. According to Müller (2006), p. 43, in 2004 and 2005

[15] Six crafts occupations (chimney sweepers and five health occupations) remained exempt from this rule for safety reasons (compare §7b HwO in its version of 29.12.2003).

about 8% of newly registered crafts businesses in the control occupations were registered under the exemption for journeymen with long work experience. However, according to Müller (2006) there is no data available regarding the application of the "Altgesellenregelung" before 2004, so we cannot assess whether these numbers constitute an increase relative to the pre-reform period. Furthermore, the new rules pertaining to journeymen with long work experience were designed in a way that may have mediated the potential reform effect on self-employment: Even after the 2003 reform, the respective journeymen still had to formally apply for an exemption from the master craftsman requirement. Although there was some regional variation, in many cases the relevant authorities were the crafts chambers themselves. Therefore, it was often fellow craftsmen (i.e. incumbents) who decided in each individual case whether an exemption from the master craftsman requirement should be granted.

Thirdly, in an additional smaller amendment to the German Crafts Code that came into force on 30. December 2003, the German government introduced the concept of minor tasks ("einfache Tätigkeiten") which only required short-term training of up to three months, were not a core business element of the related crafts occupation, and, thus, were to be freed from the master craftsman requirement. Such easy tasks had to be narrowly defined. Entry into the related crafts occupation with its full range of offered services remained subject to the master craftsman requirement.[16] The introduction of minor tasks was explicitly meant to facilitate the creation of small business start-ups. It was meant to complement the introduction of Me Inc. ("Ich AG"), a start-up subsidy for the unemployed introduced in the beginning of 2003 and abolished in August 2006 (see, for example, Deutscher Bundestag (2003c), pp. 4055–4056). However, as Müller (2006), pp. 58–59 points out, it appears that this smaller amendment to the German Crafts Code turned out to be of little practical importance.

Despite the above-mentioned changes in the regulation of entry into the control occupations, we are of the opinion that the relevant regulatory change to evaluate is the full abolishment of the master craftsman requirement in what we defined above as the treatment occupations. We therefore pool all control occupations into one control group, thereby subsuming the above-mentioned partial lowering of barriers to entry, and focus our analysis on the labor market effects of the full abolishment of the master craftsman requirement relative to the control occupations. That is, we intentionally do not choose an empirical design with several treatments of different intensity as it has been implemented in Rostam-Afschar (2014) and Runst *et al.* (2016), but follow an approach more similar to Damelang *et al.* (2016). If anything, our choice for a two-group design should lead to an underestimation of reform effects and it is, thus, a rather conservative way of approaching the evaluation of this reform.

Finally, proper application of the differences-in-differences approach also requires that no other regulatory changes or economic events which may have influenced treatment and control occupations differently occurred around the time of the reform. Indeed, in the context of the 2003 reform of the German Crafts Code two such potential confounding factors exist.

[16] Compare "Gesetz zur Änderung der Handwerksordnung und zur Förderung von Kleinunternehmen" in its version of 24. December 2003 (Bundesgesetzblatt, Teil I 2003, Nr. 66 (29.12.2003), p. 2933).

As mentioned above, in the beginning of 2003 the German government introduced Me Inc., a start-up subsidy for the unemployed (Baumgartner and Caliendo (2008)). In June 2006 the Me Inc. program was abolished, and a new start-up subsidy program was introduced in August 2006 under the name "Gründungszuschuss" (Caliendo *et al.* (2009)). A concern could be that the introduction of the Me Inc. start-up subsidy in 2003 may have caused entries into deregulated crafts occupations which without the Me Inc. program would not have taken place. This would then introduce an upward bias into our empirical analysis of the effects of product market deregulation on the number of registered crafts enterprises and self-employment. While we cannot rule this out for the overall effect on the number of registered crafts enterprises, in Section 4.4.3 we perform robustness checks to ensure that our results on self-employment are not driven by the existence of the Me Inc. subsidy. Furthermore, since the start-up subsidy was mainly targeted at solo-entrepreneurs its existence should not have any confounding influence on our analysis of dependent employment effects.

Another regulatory change that may confound our analysis of the effects of product market deregulation on self-employment is the EU enlargement to the East in 2004. As of 1. May 2004, the German labor market was opened to self-employed from the new EU member states. Under certain conditions they were allowed to take up residence and run a business in Germany. As the results presented in (Brenke, 2008, p. 61) suggest, the subsequent inflow of Eastern European self-employed into crafts occupations was more strongly directed towards the treatment occupations. To check that this regulatory change does not confound our results on self-employment, in Section 4.4.3 we perform robustness checks showing that our results on self-employment are not driven by the EU enlargement to the East. It is also very unlikely that this regulatory change could confound our analysis of dependent employment effects, since the specific rules introduced in May 2004 only applied to the self-employed.

4.3.3 Data and Sampling

This study draws on three different sources of data to analyze the effects of product market deregulation on labor market outcomes in the German skilled crafts. We use data provided by the German Confederation of Skilled Crafts (ZDH), by the Research Data Centers of the German Federal Statistical Office and the Statistical Offices of the German Federal States, as well as by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research.

ZDH data

We use occupation-level data for the period 2000–2008 that is publicly available from the German Confederation of Skilled Crafts (ZDH) through its statistics website[17] to characterize treatment and control occupations and to analyze the link between firm entry deregulation

[17] See http://www.zdh-statistik.de.

and the number of registered crafts businesses. Since all crafts businesses are required by law to register with the crafts chambers, the data provided by the German Confederation of Skilled Crafts is ideal for our analysis. Apart from the number of registered crafts businesses, this data also includes information on the number of apprenticeship graduates and the number of master craftsman exams taken (including failed exams/repetitions). As the information is provided by the German Confederation of Skilled Crafts, we can easily identify all treatment and control occupations in the data.

Survey data

Our analysis of the relationship between product market deregulation and self-employment, as well as part of our analysis on dependent employment effects is based on the Microcensus.[18] The Microcensus is a yearly survey organized by the Federal Statistical Office of Germany to provide official statistical information on the German population's economic and social conditions. Specifically, we use the Scientific Use File version of the Microcensus waves 2000–2008, provided by the Research Data Centers of the German Federal Statistical Office and the Statistical Offices of the German Federal States, for our analysis. The Scientific Use File covers a 0.7% representative sample of all households in Germany. Answering the extensive core questionnaire is mandatory for the selected households. The households in the sample are periodically replaced by new ones and specific households or individuals cannot be traced over subsequent waves (for details see Boehle and Schimpl-Neimanns (2010)). For our analysis we create a sample of repeated cross sections covering the period 2000–2008. The base sample contains both male and female prime-aged (25–55 years) individuals who report being either self-employed, employed, unemployed, or out of the labor force, and whose reported occupation belongs to either the treatment or the control occupations.

Information on the current occupation (or, in the case of those currently unemployed or out of the labor force, information on the most recent occupation) is provided by all survey participants. The occupational information provided in the Microcensus is coded at the 3-digit level according to the classification of occupations of 1992 ("Klassifikation der Berufe 1992"), as provided by the Federal Statistical Office (Statistisches Bundesamt (1992)). We identify treatment and control occupations by carefully matching those occupations listed in the German Crafts Code with the occupations contained in the classification of occupations of 1992 at the 3-digit level. However, in some cases matching remains imperfect due to the grouping of occupations for anonymization purposes in the Microcensus. The respective occupational groups may include other occupations alongside a certain crafts occupation from either the treatment or the control group. We later perform robustness

[18] In this paper the terms self-employment and dependent employment refer to two separate groups of labor market participants. The term "self-employment" refers to individuals who report that they are currently self-employed – either as solo self-employed or as entrepreneurs with employees. In contrast to this, the term "dependent employment" refers to the group of salaried employees and workers.

checks to check whether our results are driven by certain treatment or control occupations. In the few cases where control and treatment occupations are both contained in the same grouping, the respective occupations are dropped from the sample and are, thus, completely excluded from the analysis. Overall, out of the theoretically available 29 control occupations and 53 treatment occupations, in the Microcensus data we are able to identify 25 control occupations grouped into 23 occupation IDs, as well as 37 treatment occupations grouped into 31 occupation IDs.

To increase comparability the base sample excludes civil servants, soldiers, apprentices, persons in military service or alternative civilian service, and persons helping in the family business, as well as all individuals holding a university degree or a university of applied sciences degree. We also exclude all observations with missing educational information. Overall, the base sample contains about 195,000 person-year observations. We do not use the sampling weights provided with the Microcensus data. Furthermore, while the base sample is used for some parts of the analysis, we also create a set of subsamples which are required for specific aspects of our analysis. We also aggregate the individual-level data by occupations to create an occupation-year panel. The Appendices 5.3.1 and 5.3.2 provide further technical details on our use of the Microcensus survey data, with Tables A.20 and A.22 giving an overview of all samples used in the analysis based on Microcensus data, and Tables A.21 and A.23 defining the most important variables used in the analysis.

Administrative data

For our analysis of the link between product market deregulation and dependent employment in the skilled crafts, we additionally use German administrative data from the weakly anonymized version of the Sample of Integrated Labor Market Biographies (SIAB) provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB). The data set is based on social security records and contains a large random sample of employees and workers who were dependently employed subject to social insurance contributions sometime during the period 1975–2008. It covers the employment histories of about 1.5 million individuals, supplemented by data on their benefits receipt. Employment spells also include information on the worker's current 3-digit occupation (see Dorner *et al.* (2011) for further details).

For our analysis we draw a sample of spells at the 30th of June of each year and thereby create a person-year panel covering the period 2000–2008. The base sample contains both male and female prime-aged (25–55 years) dependently employed individuals who work either in a treatment or a control occupation. In the SIAB, the occupational information is coded at the 3-digit level according to the classification of occupations of 1988 ("Klassifikation der Berufe 1988") as provided by the Federal Employment Agency (Bundesanstalt für Arbeit (1988)). At the 3-digit level this corresponds to 330 different occupations in the SIAB which we carefully match to the crafts occupations listed in the German Crafts Code. As

in the case of the Microcensus data, for some occupations matching remains imperfect due to the grouping of occupations at the 3-digit level in the SIAB data. The respective 3-digit occupations may include other 4-digit occupations alongside a certain crafts occupation from either the treatment or the control group. We later perform robustness checks to check whether our results are driven by certain treatment or control occupations. If control and treatment occupations are both contained in the same 3-digit occupation, the respective occupations are completely excluded from the analysis. In total, in the SIAB data we are able to identify 24 control and 35 treatment occupations out of the existing 29 control and 53 treatment occupations.

Regarding the type of employment, the base sample contains full-time employed and part-time employed, but it excludes apprentices and trainees, the marginally employed, home workers, as well as all individuals holding a university degree or a university of applied sciences degree. We further exclude all observations with missing information regarding the current occupation, the type of employment, or the highest educational degree. Overall, the base sample contains about 300.000 person-year observations over the period 2000–2008, which we then aggregate by occupations to create an occupation-year panel. Table A.24 once more summarizes all sampling criteria applied to the SIAB data. Table A.25 defines the most important variables used in the analysis based on administrative data.

4.4 Empirical Analysis

4.4.1 Descriptives

Our estimation approach relies on the differential comparison of a set of treatment and control occupations. Ideally, we would like the comparability of the two groups of occupations to be reflected both in their average compositional characteristics and in the compositions' evolution over time. In this section, we use the three data sets, which we introduced in Section 4.3.3, to present descriptive statistics on the treatment and control occupations, before turning to the presentation of our estimation results in the subsequent sections. We find that during the pre-reform period the two groups of crafts occupations differed with respect to the levels of their sample characteristics. Against this background, we run a series of test regressions as described in Appendix 5.3.5 to ensure that the sample composition of the two groups of crafts occupations did not evolve systematically differently over the pre-reform period. We test the samples for systematic patterns of differential year-to-year changes in compositional characteristics.

As the descriptives presented in Table 4.1, p. 82 show, in the pre-reform period 2000–2003 the treatment occupations were smaller than the control occupations in terms of the average number of registered establishments. They also exhibited a lower average number of apprenticeship graduates, and a lower average number of master craftsman exams (including exam retakes). Tables 4.2, 4.3, and A.32 give an overview of the main characteristics of

Table 4.2: Descriptives Comparing Pre- and Post-reform Period Based on Survey Data

	Treatment		Control	
	Pre	Post	Pre	Post
A. All craftsmen				
Self-employed	5.5	6.1	9.5	10.5
Dependently employed	81.5	68.3	81.2	72.0
Unemployed	5.4	12.5	6.5	11.4
Out of labor force	7.6	13.2	2.8	6.1
Age (mean)	41.1	42.4	39.3	40.7
Female	62.2	64.2	7.8	7.9
Foreign	14.4	17.4	6.6	6.8
East EU	0.0	0.3	0.0	0.1
No vocational training	34.4	36.5	10.6	11.7
Vocational training degree	65.6	63.5	89.4	88.3
Master craftsman degree	5.1	4.0	15.6	15.0
N (total)	34800	40918	49247	48555
Sample share	41.4	45.7	58.60	54.30
B. Self-employed craftsmen only				
Age (mean)	41.6	42.1	41.0	42.4
Female	30.5	33.1	2.8	2.5
Foreign	7.7	11.8	2.8	3.9
East EU	0.2	4.5	0.2	0.8
No vocational training	11.2	14.8	4.0	4.8
Vocational training degree	88.8	85.2	96.0	95.2
Master craftsman degree	33.1	24.2	58.1	52.3
With employees	43.2	35.2	61.8	57.3
Hours per week (mean)	45.4	43.1	51.9	50.6
Subsidies	0.5	0.8	0.1	0.6
N (total)	1909	2480	4658	5078
Sample share	29.1	32.8	70.9	67.2

Data source: Microcensus SUF 2000–2008. Reported numbers are given in % if not noted otherwise. Pre-reform period refers to the years 2000–2003, post-reform period refers to the years 2005–2008. East EU refers to four new EU member states of 2004: Czech Republic, Hungary, Poland, and Slovakia.

our treatment and control occupations, comparing the pre-reform to the post-reform period, based on individual-level data. Table 4.2, Panels A, B and Table 4.3, Panel C are based on survey data (Microcensus), while Table A.32, Panel D is based on administrative data (SIAB). Panel A describes the composition of the base sample containing self-employed, dependently employed, and unemployed craftsmen, as well as craftsmen who are currently out of the labor force. Panel B describes the sub-sample of self-employed craftsmen only, and Panel C describes the respective sub-sample of dependently employed craftsmen. Both over the pre-reform period and over the post-reform period we observe that treatment and control occupations differ in the levels of their average sample characteristics. The treatment occupations exhibit a relatively higher share of female craftsmen, and also a higher share of craftsmen with foreign citizenship. The share of unskilled craftsmen is higher in the treatment occupations relative to the control occupations, and the share of craftsmen holding a master craftsman certificate is lower. Over the pre-reform period the share of self-employed craftsmen was lower in the treatment occupations, but the gap is much smaller in the post-reform period. In both periods the shares of dependently employed craftsmen are fairly similar in the two occupation groups. Additionally, Panel B of Table 4.2 reveals that the share of self-employed with employees is higher in the control occupations, while the receipt of start-up subsidies is more frequent in the treatment occupations. The fact that the share of self-employed from new EU member states rose strongly over time in the treatment occupations, but not in the control occupations, emphasizes the importance of applying suitable robustness checks for the EU expansion to the East to our estimation results. Furthermore, Panel C of Table 4.3 shows that dependently employed craftsmen in the treatment occupations work less hours per week on average. Correspondingly, the share of part-time employment is relatively higher in the treatment occupations. This holds for both measures of part-time work: the one based on self-assessed part-time status, as well as the one based on the number of hours typically worked per week.[19] Table A.32, Panel D provides supplementary information on the sample of dependent employees used to construct the occupations panel based on administrative data (SIAB). The share of part-time working craftsmen in the treatment occupations is much lower in the administrative data relative to the survey data. The same holds for the share of female craftsmen. However, comparing treatment and control occupations over the pre-reform period, the main insights obtained from survey data also apply to the administrative data. The treatment occupations exhibit a relatively higher share of female craftsmen, and also a higher share of craftsmen with foreign citizenship. The share of unskilled craftsmen is higher in the treatment occupations relative to the control occupations, and the share of craftsmen holding a master craftsman certificate is lower. Again, the share of part-time working craftsmen is higher in the treatment occupations relative to the control occupations.

While we observe differences in the levels of sample characteristics across the two groups

[19] For the sake of simplicity, in the individual-level regressions we therefore only use the hours-based part-time measure to differentiate between the effects on full-time and part-time employment. However, we consider both measures in the occupation-level regressions.

Table 4.3: Descriptives Comparing Pre- and Post-reform Period Based on Survey Data (Table 4.2 Continued)

	Treatment		Control	
	Pre	Post	Pre	Post
C. Dependently employed craftsmen only				
Age (mean)	41.0	42.3	39.0	40.2
Female	61.3	61.4	7.8	7.3
Foreign	14.8	16.1	6.9	6.6
No vocational training	34.6	34.5	10.5	10.6
Vocational training degree	65.4	65.5	89.5	89.4
master craftsman degree	3.7	3.1	11.6	11.8
Hours per week (mean)	28.1	27.1	38.3	38.5
Part-time (self-assessed)	44.6	49.3	3.1	5.0
Part-time ($\leq$ 30 hours)	45.5	49.6	3.6	5.2
N (total)	27720	27209	39481	34569
Sample share	41.2	44.0	58.8	56.0

Data source: Microcensus SUF 2000–2008. Reported numbers are given in % if not noted otherwise. Pre-reform period refers to the years 2000–2003, post-reform period refers to the years 2005–2008.

of crafts occupations, our estimation approach mainly relies on the assumption that the two groups would have evolved similarly in the absence of the reform. We cannot test this counterfactual scenario, but we run a series of tests as described in Appendix 5.3.5 to check whether the composition of the two groups of crafts occupations evolved similarly over the pre-reform period. According to the summary of test results provided in Appendix 5.3.5, we only find a few systematic differences between treatment and control occupations in the absolute and relative year-to-year changes of a wide range of compositional characteristics – the most important difference being that in the base sample shown in Table 4.2, Panel A the share of part-time working craftsmen increased significantly more strongly in the treatment occupations than in the control occupations in 2000–2001 and in 2001–2002 (for further details see Appendix 5.3.5). Overall, we observe that while the treatment and control occupations differed with respect to the levels of their compositional characteristics, the two groups' compositional characteristics evolved fairly similarly over the pre-reform period.

4.4.2 Number of Registered Establishments

The main objective of the abolishment of barriers to firm entry into crafts occupations was to foster entrepreneurship and firm entry into the deregulated markets. We therefore begin our analysis of the effects of product market deregulation on labor market outcomes by studying the relationship between the 2003 reform of the German Crafts Code and growth in the number of registered crafts establishments.

Table 4.4: Occupation-level Estimation Results on Growth in the Number of Registered Establishments in Crafts Occupations

	Differences-in-differences estimation						FD estimation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TG*PR	0.069***	0.069***	0.069***	0.086***	0.060***	0.068***	
	(0.008)	(0.008)	(0.006)	(0.008)	(0.006)	(0.006)	
Treatment group	-0.017***	-0.017***					0.217***
	(0.004)	(0.004)					(0.074)
Post-reform period	0.010***						
	(0.003)						
Year fixed effects	no	yes	yes	yes	yes	yes	–
Occupation fixed effects	no	no	yes	yes	yes	yes	–
Wave 2004 included	no	no	no	yes	no	yes	no
Check: EU expansion	no	no	no	no	yes	yes	yes
Dependent variable in	– growth rates –						
N	574	574	574	656	518	592	74
Adj. R^2	0.214	0.260	0.594	0.521	0.580	0.580	0.061

Data source: ZDH, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimation results from OLS estimation with constant. DiD estimations based on occupations panel, wave 2004 excluded except in specifications (4) and (6), dependent variable: yearly growth rate of number of establishments, robust standard errors. Occupation fixed effects "yes" indicates that we have performed the respective within-transformation. Check: EU expansion "yes" indicates that we exclude treatment occupations with a high share of newly registered establishments coming from new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. FD estimation based on two-period panel, dependent variable: growth rate of number of establishments (comparing pre- and post-reform period), wave 2004 excluded, robust standard errors.

Table 4.4 provides the estimation results of our differences-in-differences estimations based on an occupations panel. The coefficient on $TG * PR$ reflects the reform effect. According to column (1), on average the abolishment of the master craftsman requirement was associated with an increase in yearly growth in the number of registered crafts establishments by about 6.9 percentage points relative to the control occupations. The estimate barely changes if we control for year and occupation fixed effects in columns (2) and (3). This estimate is economically large given that the average yearly growth rate in the number of registered crafts establishments in the treatment occupations was negative and amounted to -2.6% over the pre-reform period 2000–2003. An estimated effect of about 6.9 percentage points would roughly correspond to a total of about 5,300 additionally registered crafts establishments per year. If we include the year 2004 in our estimation sample, the coefficient even increases to 8.6 percentage points and remains highly statistically significant.

However, one has to keep in mind the potentially confounding factors which we mentioned in Section 4.3.2. According to Müller (2006) and Brenke (2008), the EU enlargement to the East may at least partly explain the large estimates. As Müller (2006), pp. 64–65 reports for the post-reform years 2004 and 2005, the inflow of entrepreneurs from the new EU member states into German crafts occupations was focused on certain specific treatment occupations, and it barely affected the control occupations at all. If we drop from our estimation sample those two treatment occupations that are responsible for about 90% of all newly registered crafts businesses from the new EU member states, the coefficient of interest

actually increases to about 7.5 percentage points (not reported in Table 4.4). Column (5) in Table 4.4 reports a stricter version of this robustness check where we exclude from the sample all treatment occupations that in 2004 and 2005 had substantial shares of newly registered establishments from the new EU member states of more than about 3% according to Müller (2006), p. 186. Here, we find that the estimate reduces to about 6.0 percentage points, but remains highly statistically significant. If we then, again, also include the year 2004 in our estimation sample, the estimated growth differential amounts to 6.8 percentage points. Thus, even if we control for the inflow of entrepreneurs stimulated by the EU expansion to the East, we still find that the abolishment of barriers to firm entry in the treatment occupations was associated with a substantial increase in growth in the average number of registered crafts establishments by about 6.0–6.8 percentage points. This would roughly correspond to a total of about 2600–3000 additionally registered crafts establishments per year. These results are also largely robust to sequentially excluding single years or occupations from the estimation sample. When we exclude the year 2003 from the estimation sample, the estimated coefficient drops to its lowest value at 5.5 percentage points .

As recommended by Bertrand *et al.* (2004), we also run first-differences estimations on a two-period version of our occupations panel to compare average pre- versus post-reform growth rates in the number of registered crafts establishments. The coefficient on the treatment group dummy in column (7) gives us the effect of interest. We find that the average growth in the number of registered crafts establishments was about 21.7 percentage points higher in the treatment occupations relative to the control occupations. The effect is highly statistically significant and it is mostly robust to sequentially excluding single occupations from the estimation sample. In one case, if we exclude the treatment occupation *container and apparatus makers* from the estimation sample, the estimate drops to 16 percentage points but remains highly statistically significant.

4.4.3 Self-employment

The occupation-level results presented in the previous section suggest that the lowering of barriers to firm entry led to increased growth in the number of registered crafts establishments in the treatment relative to the control occupations. However, so far we have not been able to exclude one further potentially confounding factor from our analysis: The introduction of the start-up subsidy Me Inc. in 2003 may explain at least part of the estimated relationship. Unfortunately, we cannot rule out this latter potentially confounding factor based on aggregate ZDH data. In this section, we perform individual-level as well as occupation-level regressions to study the relationship between the abolishment of barriers to firm entry and self-employment in the skilled crafts. In Germany, self-employed who offer their services in one of the markets regulated by the German Crafts Code have to register their business with the crafts chambers. Therefore, this labor market outcome should be the one most closely related to our analysis of growth in the number of registered crafts businesses. Furthermore,

Table 4.5: Occupation-level Differences-in-differences Results on the Number of Self-employed in Crafts Occupations

	Differences-in-differences estimation on occupations panel						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TG*PR	0.722	2.978	2.978**	2.728**	2.062*	3.061**	2.145*
	(9.723)	(7.409)	(1.389)	(1.308)	(1.229)	(1.375)	(1.216)
Treatment group	-32.955***	-34.083***					
	(6.497)	(5.765)					
Post-reform period	4.565						
	(8.943)						
Year fixed effects	no	yes	yes	yes	yes	yes	yes
Occupation fixed effects	no	no	yes	yes	yes	yes	yes
Wave 2004 included	no	no	no	yes	no	no	no
Check: EU expansion	no	no	no	no	yes	no	yes
Check: Subsidies	no	no	no	no	no	yes	yes
Dependent variable in	– levels –						
N	400	400	400	450	400	400	400
Adj. R^2	0.107	0.096	0.975	0.976	0.979	0.975	0.979

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on occupations panel as described in Table A.20, dependent variable: number of self-employed, wave 2004 excluded, robust standard errors. Check: EU expansion "yes" indicates that we exclude individuals coming from four new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. Check: Subsidies "yes" indicates that we exclude individuals who presumably receive Me Inc. start-up subsidies.

the use of individual-level data allows us to run robustness checks for the receipt of start-up subsidies.

At the occupational level, we consider the relationship between the reform and the number of self-employed craftsmen. At the individual level, we analyze several outcomes: the probability of being self-employed among all employed craftsmen, as well as the probability of being self-employed among all employed craftsmen, unemployed craftsmen and those craftsmen who are currently out of the labor force. In addition, we consider the probability of entry into self-employment, as well as the probability of exit out of self-employment. We also study how the reform relates to the probability of being newly self-employed among all self-employed craftsmen. We consider as "newly self-employed" all those craftsmen who just started their current self-employment in the year in which the survey was taken. All results on self-employment are based on survey data from the Microcensus SUF 2000–2008. The technical Appendix 5.3.1 provides detailed information on the definitions of outcome variables and samples. It also provides an overview of further important variables used in the analysis.

Table 4.5 provides the estimation results of our differences-in-differences estimations based on an occupations panel. The coefficient on $TG * PR$ describes the relationship between the lowering of barriers to entry into crafts occupations and the number of self-employed. The specification with year and occupation fixed effects in column (3) indicates that, on average, the abolishment of the master craftsman requirement was associated with an

increase in the number of self-employed craftsmen by about 3 persons relative to the control occupations.[20] If we include the year 2004 in our estimation sample, the estimate reduces to about 2.7 and remains statistically significant. In contrast to what we observed in the previous section regarding the 2004 increase in the number of registered establishments, this reduction suggests that while the reform came into force on 1. January 2004, it took some time for craftsmen to react to the regulatory changes. However, one has to keep in mind that the Microcensus survey data was collected in spring 2004, which may explain that we do not observe a substantial rise in the number of self-employed in 2004 based on Microcensus data. Furthermore, to check whether the estimates are confounded by the 2003 introduction of the Me Inc. start-up subsidy, we construct a dummy variable that indicates the presumable receipt of Me Inc. start-up subsidies (for details on the variable definition see Table A.21).[21] As a robustness check, we can now drop all self-employed who presumably receive the Me Inc. start-up subsidy from the estimation sample. If we implement checks both for the EU expansion to the East and for receipt of the Me Inc. start-up subsidy, the estimate reduces to about 2.1 in column (7). Compared to the average number of self-employed craftsmen per treatment occupation, which amounted to about 18.4 over the pre-reform period, the estimated effect roughly corresponds to a relative increase by about 12%. If we sequentially exclude single occupations from the estimation sample, we find that in some cases the effect of interest further decreases in size and turns statistically insignificant. This also happens if we exclude either the year 2000 or the year 2008 from the estimation sample.

At the occupational level, we additionally perform first-differences estimations on a two-period version of our occupations panel to compare growth in the average pre- versus post-reform numbers of self-employed craftsmen. The coefficient on the treatment group dummy in Table 4.6, p. 102 gives us the effect of interest. When we compare the average growth rates spanning the reform period between treatment and control occupations, we find that growth in the deregulated crafts occupations was about 25.5 percentage points higher in the treatment occupations, on average. The effect is statistically significant at the 10%-level. According to Table 4.6, if we apply checks for the EU expansion to the East and the Me Inc. subsidies, the estimated coefficient only slightly diminishes to 24.1 percentage points and remains statistically significant at the 10%-level. However, when we sequentially exclude single occupations from the estimation sample, we find that the magnitude of the estimates varies in the range of about 15–30 percentage points, and in many of the robustness regressions the estimates of interest are statistically insignificant at significance levels of mostly 11–15%. In the most extreme case, when we exclude the treatment occupation *decorative metalworkers* from the estimation sample, the estimate drops to 0.153 and turns statistically insignificant.

[20] Due to a few year-occupation cells with zero self-employed, we cannot calculate yearly growth rates. Therefore, we present results for the outcome variable in levels. This is feasible, since Figure 4.4 shows that the number of self-employed in the treatment and the control occupations followed similar trends over the pre-reform period even if one considers the outcome in levels (see left panel of Figure 4.4).

[21] This is a modified version of the subsidies variable used in Rostam-Afschar (2014).

Table 4.6: Occupation-level First-differences Results on Growth in the Number of Self-employed in Crafts Occupations

	First-differences estimation on occupations panel			
	(1)	(2)	(3)	(4)
Treatment group	0.255*	0.257*	0.239*	0.241*
	(0.142)	(0.143)	(0.140)	(0.141)
Year fixed effects	–	–	–	–
Occupation fixed effects	–	–	–	–
Wave 2004 included	no	no	no	no
Check: EU expansion	no	no	yes	yes
Check: Subsidies	no	yes	no	yes
Dependent variable in	– growth rates –			
N	50	50	50	50
Adj. R2	0.039	0.039	0.033	0.034

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All FD estimation results from OLS estimation with constant based on two-period panel, dependent variable: growth rate of number of self-employed (comparing pre- and post-reform period), wave 2004 excluded, robust standard errors. Check: EU expansion "yes" indicates that we exclude individuals coming from four new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. Check: Subsidies "yes" indicates that we exclude individuals who presumably receive Me Inc. start-up subsidies.

At the aggregate level, the results suggest that the abolishment of barriers to firm entry into crafts occupations had a positive effect on self-employment. However, with the weak robustness of the results in mind, we now turn to the individual-level analysis. Table 4.7 gives an overview of the results of differences-in-differences estimations based on individual-level data. The coefficient on $PR * TG$ describes the relationship between the 2003 reform of the German Crafts Code and individual-level self-employment outcomes. Surprisingly, in column (1) we find no relationship between the reform and the probability of being self-employed among all employed craftsmen. When we turn to the probability of being self-employed in a sample consisting of craftsmen who are self-employed, dependently employed, unemployed, or out of the labor force, the results in column (2) also suggest that the reform was neutral. The results for the probability of entry into or out of self-employment are similarly neutral. Our results on these four outcome variables are largely robust against a number of robustness checks, such as sequentially excluding single years, occupations, federal states, or five-year age groups from the estimation sample. Furthermore, the results presented in columns (1)–(4) already incorporate the robustness checks regarding the EU expansion to the East and the start-up subsidy Me Inc.

Additionally, we estimate differences-in-differences-in-differences (DiDiD) specifications by interacting dummy variables for certain individual-level characteristics with the terms PR, TG and $PR * TG$, respectively. Table A.33 shows selected results of the DiDiD-estimations, with columns (1) and (4) repeating the differences-in-differences results from Table 4.7. We observe a few cases of effect heterogeneity for female craftsmen and for self-employed

Table 4.7: Individual-level Differences-in-differences Results on Self-employment in Crafts Occupations

	Differences-in-differences estimation				
	(1)	(2)	(3)	(4)	(5)
	Share	Probability	Entry	Exit	New self-employment
TG * PR	-0.001	-0.001	-0.001	-0.012	0.019**
	(0.009)	(0.004)	(0.002)	(0.027)	(0.008)
Female	-0.083***	-0.077***	-0.009***	0.106**	0.021**
	(0.015)	(0.012)	(0.002)	(0.052)	(0.008)
Age	0.020***	0.019***	0.002***	-0.021***	-0.016***
	(0.004)	(0.004)	(0.001)	(0.006)	(0.003)
Age^2	-0.000***	-0.000***	-0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Foreign	-0.011**	-0.010**	-0.000	0.038***	0.022*
	(0.005)	(0.004)	(0.001)	(0.012)	(0.013)
No vocational training	-0.025**	-0.025***	-0.003**	0.046***	0.014*
	(0.009)	(0.009)	(0.001)	(0.015)	(0.008)
Year fixed effects	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes
Wave 2004 included	no	no	no	no	no
Check: EU expansion	yes	yes	yes	no	yes
Check: Subsidies	yes	yes	yes	no	yes
Dependent variable in	– levels –				
N	145,153	173,294	128,120	9,050	13,663
Adj. R^2	0.018	0.017	0.001	0.028	0.021

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on individual-level data, different estimation samples as described in Table A.20, dependent dummy variables: see column title and description in Table A.21, wave 2004 excluded, standard errors clustered at occupation level. Other controls include age, age^2, dummy variables for foreign citizenship, no vocational training. Occupation fixed effects "yes" indicates that the model has been within-transformed with respect to occupation-specific means. Check: EU expansion "yes" indicates that we exclude individuals coming from four new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. Check: Subsidies "yes" indicates that we exclude individuals who presumably receive Me Inc. start-up subsidies.

craftsmen with employees. Table A.33, column (2) suggests that female craftsmen profited from the reform relative to male craftsmen regarding the probability of being self-employed (see the coefficient on Female * TG * PR). Furthermore, according to column (3) the probability of being self-employed increased more strongly for self-employed with employees relative to that of solo self-employed (see the coefficient on With empl. * TG * PR). However, these differential effects are only statistically significant in the specifications where we incorporate the checks for the EU expansion to the East and for the Me Inc. start-up subsidy. If we loosen our sampling restrictions, the respective coefficients turn statistically insignificant. Interestingly, Table A.33, column (5) indicates that the probability of entry into self-employment decreased more strongly for self-employed with employees relative to that of solo self-employed. This differential effect is statistically significant whether or not we incorporate the other robustness checks for the EU expansion to the East and the start-up subsidy Me Inc. However, this seems to contradict the differential results we just presented for the probability of being self-employed. Unfortunately, due to data limitations we cannot perform similar DiDiD regressions for the probability of exit out of self-employment.

While the results in Table 4.7 suggest that the 2003 reform of the German Crafts Code was neutral with respect to many individual-level self-employment outcomes, the table's last column displays a positive and statistically significant coefficient on the probability of being newly self-employed. In addition to this, Table 4.8 provides detailed regression results on the probability of being newly self-employed. Column (4) shows the base specification with an estimated effect of about 2 percentage points that is statistically significant at the $\alpha = 5\%$ level. The application of robustness checks for the EU expansion to the East and the start-up subsidy Me Inc. slightly reduces the estimated coefficient in size. The coefficient also decreases if we include the wave 2004 in our estimation sample, but the estimate remains statistically significant. We find no statistically significant differential effects across demographic subgroups.

An estimated effect of 1.9 percentage points would actually be sizable. Given the average pre-reform share of new self-employment of about 3.6% in the treatment occupations, an additional increase by 1.9 percentage points would correspond to a relative increase by about 53%. However, when we conduct a series of robustness checks, such as sequentially excluding single years, occupations, federal states, five-year age groups, or demographic subgroups from the estimation sample, we find that this estimation result is less robust than the other results presented above in Table 4.7. When we exclude either the federal state of Lower Saxony or the year 2005 from the estimation sample, the estimate even drops as low as 0.015 and turns statistically insignificant.

To sum up our results on self-employment, at the aggregate level we find that the abolishment of barriers to firm entry into the skilled crafts was associated with a both economically and statistically significant increase in the number of self-employed craftsmen in the treatment occupations relative to the control occupations. However, this occupation-level relationship is hardly reflected in the individual-level analysis of self-employment probabilities. We

Table 4.8: Individual-level Differences-in-differences Results on the Probability of Being Newly Self-employed in Crafts Occupations

	Differences-in-differences estimation for new self-employment					
	(1)	(2)	(3)	(4)	(5)	(6)
TG*PR	0.027***	0.026***	0.025***	0.020**	0.019**	0.016**
	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)
Treatment group	-0.003	-0.003				
	(0.010)	(0.010)				
Post-reform period	0.005					
	(0.006)					
Female				0.022***	0.021**	0.021***
				(0.008)	(0.008)	(0.007)
Age				-0.017***	-0.016***	-0.018***
				(0.003)	(0.003)	(0.003)
Age^2				0.000***	0.000***	0.000***
				(0.000)	(0.000)	(0.000)
Foreign citizenship				0.032*	0.022*	0.034**
				(0.016)	(0.013)	(0.015)
No vocational training				0.010	0.014*	0.013
				(0.009)	(0.008)	(0.008)
Year fixed effects	no	yes	yes	yes	yes	yes
Occupation fixed effects	no	no	yes	yes	yes	yes
Wave 2004 included	no	no	no	no	no	yes
Check: EU expansion	no	no	no	no	yes	no
Check: Subsidies	no	no	no	no	yes	no
Dependent variable in	– levels –					
N	13,886	13,886	13,886	13,886	13,663	15,526
Adj. R^2	0.002	0.004	0.003	0.023	0.021	0.023

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on individual-level data, estimation sample *new self-employment* as described in Table A.20, dependent dummy variables: new self-employment as described in Table A.21, wave 2004 excluded, standard errors clustered at occupation level. Occupation fixed effects "yes" indicates that the model has been within-transformed with respect to occupation-specific means. Check: EU expansion "yes" indicates that we exclude individuals coming from four new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. Check: Subsidies "yes" indicates that we exclude individuals who presumably receive Me Inc. start-up subsidies.

only observe a non-zero relationship between the reform and the probability of being newly self-employed. However, we must keep in mind that the checks regarding the credibility of the common trends assumption revealed a statistically significant difference in pre-reform trends for exactly this outcome variable.

4.4.4 Dependent Employment

The proponents of the 2003 reform of the German Crafts Code argued that the barriers to firm entry created by the master craftsman requirement distorted competition, which, ultimately, also had adverse effects on dependent employment in the skilled crafts. Accordingly, the proponents of the reform expected that the abolishment of the master craftsman requirement should lead to higher levels of dependent employment in the deregulated markets. As we have mentioned in Section 4.1, this expectation is also in line with economic theory.

Table 4.9: Occupation-level Differences-in-differences Results on Growth in Dependent Employment Outcomes in Crafts Occupations Based on Survey Data

	Differences-in-differences estimation on occupations panel				
	(1) Total	(2) FTE	(3) FTE (SIAB)	(4) Full-time (> 30 h)	(5) Full-time (self-rep.)
TG*PR	-0.013	-0.014	-0.015	-0.006	-0.006
	(0.030)	(0.029)	(0.030)	(0.030)	(0.030)
Year fixed effects	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes
Wave 2004 included	no	no	no	no	no
Dependent variable in	– growth rates –				
N	357	357	357	357	357
Adj. R^2	-0.048	-0.039	-0.022	-0.039	-0.043

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on occupations panel as described in Table A.22, dependent variables: yearly growth rate of respective variable (see column title and description in Table A.23), wave 2004 excluded, robust standard errors.

In this section, we therefore study how the abolishment of barriers to firm entry into the treatment occupations was related to a wide range of measures of dependent employment. Most results on dependent employment are based on survey data from the Microcensus. The technical Appendix 5.3.2 explains the definition of the respective outcome variables, important covariates, and samples. We supplement our occupation-level results on dependent employment by providing further empirical evidence based on administrative data (SIAB). Details on the definition of the respective outcome variables and samples can also be found in the technical Appendix 5.3.2.

Table 4.9 provides differences-in-differences estimation results on growth in dependent employment outcomes which have been aggregated at the occupational level. All outcome variables are measured in conventional growth rates, and the coefficient on $TG*PR$ reflects the effect of interest. We consider how the abolishment of barriers to firm entry into the treatment occupations was related to growth in the total number of dependently employed craftsmen (including both part-time and full-time employed) in column (1). Furthermore, we run the estimations for growth in two different kinds of full-time equivalents: In column (2), full-time equivalents are based on weighting observations by the exact number of self-reported typical weekly working hours. In column (3), we use full-time equivalents constructed to mimic the weighting procedures underlying the SIAB-based full-time equivalents. In addition to this, we consider growth in the number of full-time employed craftsmen: In column (4), the full-time measure is based on the worker-specific typical weekly working hours, whereas in column (5) full-time status is self-reported.[22] For the wide range of

[22]Unfortunately, we cannot provide results on growth in part-time employment. We cannot compute growth

Table 4.10: Occupation-level First-differences Results on Growth in Dependent Employment Outcomes in Crafts Occupations Based on Survey Data

	First-differences estimation				
	(1) Total	(2) FTE	(3) FTE (SIAB)	(4) Full-time (> 30 h)	(5) Full-time (self-rep.)
Treatment group	0.014	0.014	0.016	0.008	0.005
	(0.046)	(0.047)	(0.048)	(0.047)	(0.047)
Year fixed effects	–	–	–	–	–
Occupation fixed effects	–	–	–	–	–
Wave 2004 included	no	no	no	no	no
Dependent variable in	– growth rates –				
N	51	51	51	51	51
Adj. R^2	-0.019	-0.019	-0.018	-0.020	-0.020

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All FD estimation results from OLS estimation with constant based on two-period panel, dependent variables: growth rate of respective variable (comparing pre- and post-reform period), wave 2004 excluded, robust standard errors.

aggregate employment outcomes presented in Table 4.9, we find no statistically significant reform effects. The estimated effects on growth in the total number of dependently employed, in full-time equivalents, and in full-time employment are negative and very small in absolute size. All estimated coefficients are statistically insignificant. We also apply a series of robustness checks to these regressions. When we include the year 2004 in our estimation sample, all estimated coefficients are even closer to zero and remain statistically insignificant. The results are largely robust to the exclusion of single years or single occupations from the estimation sample. In some cases, the robustness checks yield very small positive but still statistically insignificant coefficients.

In addition to this, we run first-differences estimations on a two-period version of our occupations panel to compare growth in average pre- versus post-reform dependent employment outcomes. The coefficient on the treatment group dummy in Table 4.10 gives us the effect of interest. We find that for a wide range of outcomes growth rates spanning the introduction of the reform were similar in the treatment and the control occupations. The coefficients of interest for the total number of dependently employed, the two measures of full-time equivalents, and the two measures of full-time employed, are all positive but rather small in absolute size and statistically insignificant. These results are, again, largely robust to sequentially excluding single occupations from the estimation sample.

rates due to a number of year-occupation cells with zero part-time employees. Furthermore, we cannot present estimation results on the part-time variables in levels, since the graphical evidence summarized in Section 4.3.1 clearly indicates that the dependent employment outcomes only followed parallel pre-reform trends in relative terms (right panels of graphs) but not if we consider absolute levels (left panels of graphs). However, part-time employment enters the analysis both through the total number of dependently employed craftsmen, as well as through the two different types of full-time equivalents.

Table 4.11: Occupation-level Estimation Results on Growth in Dependent Employment Outcomes in Crafts Occupations Based on Administrative Data

	Differences-in-differences estimation			First-differences estimation		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	FTE	Full-time	Total	FTE	Full-time
TG*PR	-0.027*	-0.029*	-0.033**			
	(0.015)	(0.015)	(0.016)			
TG				-0.052*	-0.056*	-0.064**
				(0.030)	(0.031)	(0.032)
Year fixed effects	yes	yes	yes	–	–	–
Occupation fixed effects	yes	yes	yes	–	–	–
Wave 2004 included	no	no	no	no	no	no
Dependent variable in	– growth rates –					
N	413	413	413	59	59	59
Adj. R^2	-0.009	-0.007	-0.013	0.029	0.034	0.044

Data source: SIAB 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimation results from OLS estimation with constant. DiD estimations based on occupations panel as described in Table A.24, dependent variables: yearly growth rate of respective variable (see column title and description in Table A.25), wave 2004 excluded, robust standard errors. FD estimation based on two-period panel as described in Table A.24, dependent variables: growth rate of respective variable (comparing pre- and post-reform period), wave 2004 excluded, robust standard errors.

Table 4.11 provides further occupation-level results on growth in dependent employment outcomes based on administrative data (SIAB). Again, all outcome variables are measured in conventional growth rates. In column (1), we run a differences-in-differences regression to analyze how the abolishment of barriers to firm entry into the treatment occupations was related to growth in the total number of dependently employed craftsmen (including both part-time and full-time employed). Furthermore, we run differences-in-differences estimations for growth in full-time equivalents in column (2), and for growth in the number of full-time employed craftsmen in column (3). Columns (4)–(6) contain the corresponding first-differences estimation results. The estimated differences-in-differences results on growth in the total number of dependently employed, in full-time equivalents, and in the number full-time employed are negative and statistically significant. The results indicate that growth in dependent employment was about 3 percentage points lower in the treatment occupations relative to the control occupations. Given that during the pre-reform period the average yearly growth rates in the respective outcome variables were negative and amounted to about -3.2% in the treatment occupations, the differences-in-differences results are sizable. The estimates are also qualitatively robust to including the year 2004 in the estimation sample. According to Table 4.11, the first-differences estimates are more pronounced than the respective differences-in-differences results. The results in columns (4)–(6) also suggest negative effects of the abolishment of barriers to firm entry on dependent employment. However, if we sequentially exclude single occupations from the estimation sample, the first-differences estimates decrease in absolute size to a minimum of about -4 percentage points and turn statistically insignificant in several cases.

At the individual level, we analyze how the abolishment of barriers to firm entry was related

Table 4.12: Individual-level Differences-in-differences(-in-differences) Results on Dependent Employment in Crafts Occupations Based on Survey Data

	Differences-in-differences(-in-differences) estimation						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Probability	Probability	Entry	Entry	Exit	New employment	
TG*PR	-0.046***	-0.065*	-0.008*	-0.007**	0.017***	-0.002	-0.006*
	(0.015)	(0.034)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
Part-time * TG* PR		0.062*		-0.039			-0.031
		(0.034)		(0.026)			(0.025)
Part-time * TG		0.252***		-0.015			-0.050***
		(0.027)		(0.019)			(0.014)
Part-time * PR		0.096***		0.049*			0.046*
		(0.011)		(0.026)			(0.025)
Part-time		0.254***		0.143***			0.084***
		(0.021)		(0.019)			(0.014)
Female	-0.026	-0.144***	0.007*	-0.024**	-0.015	-0.007	-0.025*
	(0.037)	(0.020)	(0.004)	(0.011)	(0.016)	(0.012)	(0.013)
Age	0.008**	0.004	-0.007***	-0.008***	-0.008***	-0.009***	-0.009***
	(0.004)	(0.004)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Age2	-0.000***	-0.000*	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Foreign	-0.018**	-0.003	0.009***	0.012***	0.021***	0.016***	0.017***
	(0.008)	(0.006)	(0.002)	(0.004)	(0.004)	(0.003)	(0.004)
No vocational training	-0.049***	-0.041***	-0.006	-0.004	0.007***	0.001	-0.001
	(0.004)	(0.006)	(0.006)	(0.004)	(0.002)	(0.003)	(0.003)
Year fixed effects	yes	yes	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes	yes	yes
Wave 2004 included	no	no	no	no	no	no	no
Dependent variable in	– levels –						
N	173,520	173,520	128,314	128,314	90,243	127,015	127,015
Adj. R2	0.027	0.162	0.005	0.041	0.004	0.010	0.015

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on individual-level data, different estimation samples as described in Table A.22, dependent dummy variables: see column title and description in Table A.23, wave 2004 excluded, standard errors clustered at occupation level. Occupation fixed effects "yes" indicates that the model has been within-transformed with respect to occupation-specific means.

to dependent employment probabilities. We consider the probability of being dependently employed, the probability of entry into dependent employment, as well as the probability of exit out of dependent employment based on the survey data set Microcensus. We also study the probability of being newly dependently employed among all dependently employed craftsmen. We define as "newly employed" all those employees who just started their current job in the year in which the survey was taken. As far as possible, we also analyze whether there are differential effects for part-time employed craftsmen (based on the 30-hours-cutoff version of the part-time dummy).[23]

Table 4.12 provides an overview of the main individual-level differences-in-differences results on dependent employment. According to column (1), the probability of being dependently employed significantly decreased by 4.6 percentage points in the treatment occupations relative to the control occupations. Given that the average pre-reform probability

[23] We do not have sufficient information on the employment conditions one year before the time of the survey to study interaction effects for the probability of exit out of dependent employment.

of being dependently employed in the crafts occupations was 81.5% (compare Table 4.2, Panel A), this is roughly equivalent to a relative decrease of about 5.6%. This negative estimate on the probability of being dependently employed corresponds to a 0.8 percentage point decrease in the entry probability in column (3), as well as a 1.7 percentage point increase in the exit probability in column (5). We do not observe a statistically significant effect on the probability of being newly dependently employed in the deregulated markets. Our results on these four outcome variables are largely robust to a number of robustness checks, such as sequentially excluding single years, occupations, federal states, or five-year age groups from the estimation sample. If we include the year 2004 in the estimation sample, the estimation results become a bit less pronounced but do not change statistical significance. Furthermore, DiDiD estimation results suggest that the negative entry effect was even more pronounced for foreigners, and that the exit effect was more pronounced for female craftsmen (estimates not reported in tables).

In a further set of DiDiD regressions, we include interactions with the part-time dummy to allow for differential effects for part-time employees. We find that the estimated coefficients on $TG*PR$, which then reflect the relationship between the abolishment of barriers to firm entry and dependent employment probabilities for full-time employed, are qualitatively similar to the results described above. Furthermore, we find a statistically significant differential effect for part-time employed in the case of only one outcome. The results provided in Table 4.12, column (2) suggest that for part-time employees the probability of being dependently employed decreased much less than for full-time employees. Unfortunately, due to data limitations we cannot investigate effect heterogeneity for the probability of exit out of dependent employment.

4.4.5 Discussion of Empirical Results

In the previous sections we have presented estimation results on the relationship between the abolishment of barriers to firm entry into crafts occupations and the number of registered crafts establishments, as well as a wide range of employment outcomes at both the individual level and the occupational level. The initial objective of our analysis was to identify the causal effects of product market deregulation on individual labor-market outcomes based on a differences-in-differences analysis. The crafts occupations included in the treatment and the control group were all subject to the same regulatory framework until the end of 2003. This should have made them largely comparable and, thus, suitable for the application of a differences-in-differences approach. However, as extensive checks have revealed, the common trends assumption implied by the differences-in-differences approach is not fully plausible for the crafts occupations in our sample. This is reflected by graphical evidence on the pre-reform period, as well as by statistically significantly different pre-reform trends in some of the outcomes variables. Most importantly, the placebo test regressions reveal a substantial degree of pre-reform heterogeneity in the evolution of employment outcomes

in the treatment and control occupations. Furthermore, both for self-employment and for dependent employment we observe different, even partially contradictory results when we compare occupation-level and individual-level estimates. This further complicates the overall interpretation of our results.[24]

In addition to these findings, our insights into the institutional background of the 2003 reform of the German Crafts Code contribute to the view that the treatment control occupations are not sufficiently comparable. As we have explained in Section 4.2, the deregulated occupations constitute a non-random sample of all crafts occupations. We take account of this by fully excluding from the analysis those 12 crafts occupations which were reassigned from becoming deregulated to remaining regulated in 2003. However, this sampling rule cannot fully solve the potential distortion of results arising from non-random assignment into treatment. If, for example, reassignment was achieved for those 12 crafts occupations for which the negotiation efforts were most economically rewarding through the upholding of incumbents' economic rents, then the remaining 53 treatment occupations would constitute a negative selection. These 53 occupations would then be the occupations with relatively worse prospects of economic performance – or these could be the occupations where barriers to firm entry induced by the master craftsman requirement were relatively less binding or economically less relevant during the pre-reform period. Although both scenarios are hypothetical, they illustrate how non-random assignment into treatment could distort our estimation results.

Given the problems revealed both by our investigation of the institutional background of the reform and by the extensive empirical checks, we must conclude that we cannot interpret the results presented in the previous sections as reflecting the causal effects of the abolishment of barriers to firm entry on labor market outcomes in the German skilled crafts. This paper therefore mainly documents our analysis of the reform, and we are very careful with the overall interpretation of our results. However, the estimation results are still suggestive of a certain direction of the reform effects. We find that after the abolishment of barriers to firm entry, growth in the number of registered crafts establishments was significantly higher in the treatment occupations relative to the control occupations. This result is robust to taking account of the EU expansion to the East in 2005. Our analysis in Section 4.4.3 suggests that at the aggregate level the increase in the number of registered crafts establishments translated into an increase in the number of self-employed craftsmen. This holds even if we include robustness checks for two potentially confounding factors: the receipt of start-up subsidies, as well as the EU expansion to the East in 2005. However, at the individual level the differences-in-differences estimations suggest that the reform was largely neutral with respect to self-employment probabilities and the self-employment share. Only the probability of being newly self-employed shows a statistically significant positive coefficient of interest, but the respective pre-reform trend regression and further robustness checks cast doubts on

[24] These differences between occupation-level and individual-level results continue to exist even if we consider individual-level specifications that do not include any other covariates besides year and occupation fixed effects.

the reliability of this specific result.

Our individual-level results on self-employment differ from the results provided in Rostam-Afschar (2014) and Runst *et al.* (2016), who, based on the same data set, find a positive reform effect on the probability of being self-employed, as well as on the probability of entry into self-employment. However, one has to keep in mind that our empirical design differs substantially from that chosen by Rostam-Afschar (2014) and Runst *et al.* (2016) – for example with respect to the definition of treatment and control groups and the period of observation. Similar to Damelang *et al.* (2016), we group all crafts occupations that remained subject to the master craftsman requirement into one control group, irrespective of whether or not these occupations experienced a partial lowering of barriers to firm entry trough the strengthening of exemptions from the master craftsman requirement for journeymen with long work experience. Interestingly, similar to Runst *et al.* (2016) our analysis suggests a positive reform effect on the probability of being newly self-employed, which can be seen an alternative measure of entry into-self-employment. However, as we have mentioned above the estimate is not fully reliable. Furthermore, previous research on the effects of product market deregulation has found that the lowering of barriers to firm entry tends to foster so-called "marginal self-employment". This means that the new entrepreneurs are typically less well prepared, less well educated, and their start-ups are typically smaller and survive for shorter time periods (Branstetter *et al.* (2013)). In our analysis of self-employment, we check for effect heterogeneity with respect to the individual-level characteristics of the self-employed. In contrast to Rostam-Afschar (2014), we do not find differential reform effects for low-skilled self-employed, which would be suggestive of a tendency towards marginal self-employment. However, our results suggest that female craftsmen profited from the reform relative to their male counterparts regarding the probability of being self-employed. This also corresponds to a higher post-reform share of female self-employed craftsmen in the treatment occupations. Regarding our results on entrepreneurship and self-employment, we conclude that while we cannot interpret the empirical results causally, the analysis at least partially corroborates the evidence provided by Rostam-Afschar (2014) and Runst *et al.* (2016) that the abolishment of barriers to firm entry into the German skilled crafts fostered entrepreneurship and self-employment in the deregulated markets. However, related studies indicate that these new entrepreneurs in the deregulated crafts occupations tended to be less well educated (Rostam-Afschar (2014); Runst *et al.* (2016)) and that their firms were relatively smaller and had lower survival rates (Müller (2016)).

To the best of our knowledge, this paper is the first to provide detailed estimation results on the relationship between the abolishment of barriers to firm entry into crafts occupations through the 2003 reform of the German Crafts Code and dependent employment in the German skilled crafts. At the occupational level, estimation results based on survey data suggest that the reform was neutral with respect to growth in dependent employment outcomes such as total dependent employment, full-time employment, and full-time equivalents. The individual-level results, which are also based on survey data, even suggest that the reform

effects on dependent employment probabilities were negative. Supplementary occupation-level results, which we obtain based on administrative data, also indicate that after the reform growth in dependent employment outcomes was significantly lower in the treatment occupations relative to the control occupations. One has to keep in mind that the test regressions, especially the placebo tests, reveal substantial pre-reform heterogeneity in the evolution of dependent employment outcomes in the treatment and the control occupations. Nevertheless, our estimation results at least suggest that the 2003 reform of the German Crafts Code did not have a positive effect on dependent employment in the deregulated markets. This is also in line with the conclusions recently drawn from a descriptive analysis by Müller (2016). Such a result would not necessarily contradict the predictions made by economic theory. Theoretical papers on the labor market effects of product market deregulation typically predict positive long-run effects on dependent employment in the respective markets. Our period of observation may simply be too short to observe such positive long-run effects. However, Müller (2016) provides further graphical evidence which suggests that even over the extended post-reform period 2005–2013 dependent employment did not evolve more positively in the deregulated crafts occupations relative to the regulated ones.[25]

4.5 Conclusions

Economic theory predicts that product market deregulation, such as the lowering of barriers to firm entry, should affect market competition and industry dynamics, as well as labor market outcomes. Cross-country comparison studies have, indeed, found that stricter product market regulations are associated with reduced entry of new firms and lower employment rates, among others. More recently, economists have exploited product market reforms within certain countries as natural experiments to study the effects of product market deregulation on labor market outcomes based on micro data. Our empirical study contributes to this second strand of the economic literature. We exploit the 2003 reform of the German Crafts Code as a natural experiment to study the relationship between the abolishment of barriers to firm entry into a substantial number of the German skilled crafts and the number of registered crafts businesses, self-employment, as well as dependent employment outcomes in the respective markets. While the labor market effects of the 2003 reform of the German Crafts Code have been studied before, to the best of our knowledge we are the first to provide detailed estimation results on dependent employment outcomes. Our study differs from the most closely related papers (Rostam-Afschar (2014); Runst *et al.* (2016)), which also evaluate the labor market effects of the respective reform, with respect to important aspects of the empirical design.

A close investigation of the institutional background and extensive empirical checks cast

[25] The stronger employment growth in the deregulated occupations, which Müller (2016) observes for the years 2010–2012, vanishes once the author excludes the deregulated occupation *building cleaners* from the sample (compare Figure 5 in Müller (2016), p. 11).

doubts on the comparability of treatment and control occupations. We conclude that the assumption of common trends is not fully plausible. There are substantial differences not only in the average levels, but also the in pre-reform evolution of some of the outcome variables. The view that regulated and deregulated crafts occupations are not sufficiently comparable is also in line with a very recent paper by Müller (2016). We therefore cannot interpret the estimation results causally, and we are careful with the overall interpretation of the results. We find that after the reform, the number of registered crafts establishments as well as the number of self-employed craftsmen increased more strongly in the deregulated occupations. The opposite holds for the number of dependently employed craftsmen. Individual-level estimations suggest a positive effect on the probability of being newly self-employed among all self-employed, and negative effects on dependent employment probabilities. While we cannot interpret the empirical results causally, our analysis at least partially corroborates the evidence provided by other recent empirical studies that the abolishment of barriers to firm entry into the German skilled crafts fostered entrepreneurship and self-employment in the deregulated markets (Rostam-Afschar (2014); Runst *et al.* (2016)) – although in light of recent findings in the related literature (Rostam-Afschar (2014); Runst *et al.* (2016); Müller (2016)) many of the additionally created firms were most likely marginal. Furthermore, the abolishment of barriers to firm entry seems not to have had a positive effect on dependent employment in the German skilled crafts.

Until the reform of the German Crafts Code came into force in the beginning of 2004, the treatment and control occupations in this analysis were subject to the same regulatory framework that strictly regulated firm entry into the respective markets through the master craftsman requirement. This initially led us to expect that the treatment and control occupations should be sufficiently comparable. Given the issues with the empirical approach, which only became apparent later on during our work on the research project, this study is illustrative of the difficulties which empirical researchers face time and again when striving to provide causal evidence on economic interdependencies.

5 Appendix

5.1 Appendix to Chapter 2: Mobility Across Firms and Occupations Among Graduates from Apprenticeship

5.1.1 Data Appendix 1: Matching of Instrumental Variables Across Different Spatial Classifications

For reasons of data anonymization, regional information in the IABS regional file is not coded at the original level of administrative districts (Kreise), but at a slightly aggregated level (grouped districts) which ensures that the dataset only contains regional units of at least 100.000 inhabitants. We aggregate all instrumental variables which are provided at the original administrative district (*Kreise*) level to the grouped-district level. In this, we weight districts by their relative size in terms of the number of inhabitants. The required key matching administrative districts to grouped districts is provided in Drews (2008), pp. 69–78.

Additionally, some of the instrumental variables, such as the labor market tightness measure, are only available at the level of employment agency districts (*Agenturbezirke*). This creates a problem, since administrative districts and agency district may overlap. Some administrative districts actually belong to four different agency districts. This is farther complicated by the grouping of administrative districts in the IABS regional file. Taking all these complications and spatial overlaps into account, based on the comparison of maps of administrative districts and agency districts we create a key matching agency districts and grouped districts in the IABS regional file. For simplification, in the case that an administrative district strongly overlaps with several agency districts, we assume that the administrative district is equally distributed across all relevant agency districts. The key takes into account changes at the administrative district level during the period 1988–2011. Furthermore, we checked that no major changes in agency districts occurred during the period 1988–2011 – changes were few and insignificant.

For the regional Probit Analysis in stage zero we define 26 districts based on the German regional policy districts (*Regierungsbezirke*). We assign each grouped administrative district in the IABS data to the corresponding government district (see table A.1). Due to missing variation for the city districts Hamburg and Bremen, and small sample size, we group the initial 30 government districts into 26 regions.

Table A.1: Regional Districts for Probit Analysis in Stage Zero.

District	Description	#Obs.
1	Schleswig-Holstein	502
2	Lueneburg and Hamburg	632
3	Weser-Ems and Bremen	772
4	Hannover	469
5	Braunschweig	372
6	Muenster	586
7	Detmold	539
8	Duesseldorf	1044
9	Arnsberg	800
10	Koeln	785
11	Kassel	331
12	Giessen	211
13	Darmstadt	643
14	Koblenz	300
15	Trier and Saarland	311
16	Rheinhessen-Pfalz	359
17	Karlsruhe	544
18	Stuttgart	926
19	Tuebingen	391
20	Freiburg	507
21	Unterfranken	377
22	Oberfranken and Oberpfalz	648
23	Mittelfranken	452
24	Niederbayern	391
25	Schwaben	519
26	Oberbayern	823
		14234

5.1.2 Data Appendix 2: Data Cleaning Procedures and Identification of Completed Apprenticeships

To identify an individual's first completed apprenticeship training, we apply a set of data cleaning procedures and restrictions to the IABS data. In order to identify whether an apprenticeship was successfully completed we need to observe a change in the education variable. Due to certain deficiencies of the education information provided by the IABS we use an imputed education variable based on imputation strategy ip1 proposed by Fitzenberger *et al.* (2006).

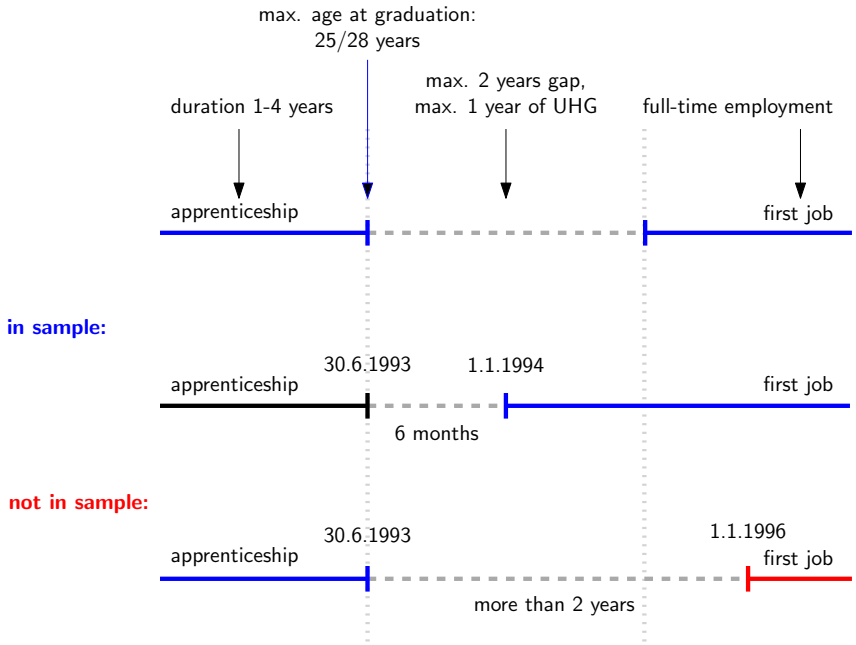
An apprenticeship episode observed in the data is identified as a person's first completed apprenticeship training if the following conditions are met. Figure A.1 provides a summary of these conditions.

1. During the apprenticeship period, the individual is still observed as holding no vocational degree.
2. The information on the training occupation is non-missing in the last training spell.
3. The duration of training is at least one year. Also, we allow for a maximum duration of four years. For the observation period, the scheduled training duration lies between two and three and a half years (depending on the occupation) with an average of about three years. However, the training duration could be further shortened due to previous educational attainments such as holding a high-school diploma (*Abitur*). During the observation period about 19% of apprenticeship durations were shortened per year (see Uhly *et al.* (2006), figures 7.1 and 7.2).
4. Age at completion of training may not be more than 25 years for persons with no more than secondary education (*Hauptschulabschluss* or *Realschulabschluss*) and 28 years for persons with high school diploma (*Abitur*).
5. The education information changes to the status "holding vocational degree" within a period of two years after graduation from apprenticeship. This two-year window is long enough to allow us to observe changes in the education variable also for individuals doing military or civilian service right after their vocational training. At the same time, limiting the analysis to a two-year window makes it very unlikely that after graduation from apprenticeship the individual obtained a second vocational degree in a different occupation through types of training unobservable to us. Most importantly, fully school based vocational training would be unobservable to us. However, during the observation period most trainees in fully school based vocational training were female.[1] Another form of training unobservable to us would be further training programs, in which case participants could apply to the employment agency for a training allowance (*Unterhaltsgeld*). Thus, as a further restriction, during the two-year

[1] According to (Bundesministerium für Bildung, Wissenschaft, Forschung und Technologie, 1997, p. 67) during the years 1992–1995 about 80% of persons in fully school based vocational training (learning an occupation outside the dual system) were female.

period individuals should not have received more than one year of training allowance.

Figure A.1: Sampling Conditions



Furthermore, the following individuals are excluded from the sample:

1. Individuals whose training occupation of identified as “occupational code 130”, since according to (Drews, 2008, p. 85) this category is also used for individuals whose training occupation is not defined yet.
2. Individuals who show earlier apprenticeship episodes lasting for longer than one year in a different occupation before the start of the main completed apprenticeship. (Shorter previous apprenticeship spells are allowed for, since they may well be internship spells that have been misclassified as apprenticeship training.)
3. Individuals for whom we observe further apprenticeship spells after graduation from apprenticeship.
4. Individuals who complete tertiary education (university degree, technical college degree) sometime during their further career.

5.1.3 Table Appendix

Table A.2: Definition of Four Mobility Groups (Number of Apprenticeship Graduates Sampled per Group in Parentheses)

		Change of firm	
		no	yes
Change of 3-digit occupation	no	stayer (n=6865)	job switcher (n=1961)
	yes	within-firm occupation switcher (n=1001)	job-and-occupation switcher (n=2187)

Table A.3: Distribution of Person-year Observations in the Wage Panel Across Four Mobility Groups by Year of Employment

	Year of employment							
Mobility type	0	1	2	3	4	5	6	7
stayer	58.41	57.72	58.46	58.98	59.13	59.16	59.29	60.11
job switcher	15.64	16.38	15.93	15.74	15.54	15.53	15.24	14.94
within-firm occ. switcher	8.41	8.33	8.46	8.44	8.48	8.48	8.65	8.80
job-and-occ. switcher	17.53	17.57	17.15	16.83	16.84	16.83	16.82	16.16
Total (N)	14225	12103	12251	12202	12141	12134	11971	11561

Notes: Sample share in the respective year of employment. Year of employment 0 refers to the year during which graduation occurred.

Table A.4: Coefficient Estimates for IV Procedure without Heterogeneous Treatment Effects (Standard Errors Clustered at Region-year-of-graduation Level)

Dependant variable: log(wage)	Short term (0-2)		Long term (3-7)	
	(1)	(2)	(3)	(4)
Job switch	-0.109***	-0.0429***	-0.123***	-0.0373*
	[0.0269]	[0.0165]	[0.0304]	[0.0193]
Within-firm occ. switch	0.232***	0.143***	0.238***	0.124***
	[0.0297]	[0.0213]	[0.0358]	[0.0256]
Job-and-occ. switch	-0.0241	-0.0333	-0.0327	-0.0305
	[0.0284]	[0.0209]	[0.0304]	[0.0234]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes
N	14225	14225	13378	13378
Adj. R-sq	0.011	0.186	0.026	0.131

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at region-interacted with year-of-graduation-level; Observations weighted by length of employment spell; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant.

Table A.5: Overidentification Tests: Number of Rejections at 1% Significance Level among 26 Regions (Standard Errors Clustered at Individual Level)

No. IVs	Short term (0-2)		Long term (3-7)	
	(1)	(2)	(3)	(4)
0. Original set of 22 IVs				
	13	8	5	7
A. 12 IVs				
	5	4	5	2
B. 9 IVs				
	2	3	1	0
C. 7 IVs				
	1	2	3	0
Fixed effects for 2-Digit training occupation	No	Yes	No	Yes

Notes: Results of the overidentification test described in section 2.4.5 for the specifications discussed in Tables 2.6 and 2.7. 7 IV's: polynomial in unemployment rate, unemployment below 25, polynomial in labor market tightness. 9 IV's: 7 IV's plus share of low-skilled and high-skilled employees. 12 IV's: 9 IV's plus exit rate from employment into unemployment, indicator small cells, interaction mobility zero. 22 IV's: 12 IV's plus further mobility shares, interaction mobility zero, indicator small cells

Table A.6: Pooled OLS Estimates Accounting for Upward and Downward Mobility

Dependant variable: log(wage)	Short term (0-2)		Long term (3-7)	
	(1)	(2)	(3)	(4)
Job switch	-0.0347***	-0.0249***	-0.0379***	-0.0220***
	[0.0056]	[0.0051]	[0.0066]	[0.0063]
Within-firm occ. switch up	0.107***	0.114***	0.0970***	0.0990***
	[0.0100]	[0.0093]	[0.0112]	[0.0110]
Within-firm occ. switch down	0.0430***	0.0534***	0.0398***	0.0470***
	[0.0110]	[0.0096]	[0.0128]	[0.0115]
Job-and-occ. switch up	-0.0123	0.00563	-0.0243**	-0.00455
	[0.0087]	[0.0084]	[0.0101]	[0.0100]
Job-and-occ. switch down	-0.0615***	-0.0653***	-0.0680***	-0.0654***
	[0.0077]	[0.0074]	[0.0086]	[0.0083]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes
N	14225	14225	13378	13378
R-sq	0.063	0.196	0.068	0.136

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at person-level; Observations weighted by length of employment spell.

Table A.7: Coefficient Estimates for Two-step IV Procedure (no Heterogeneous Treatment Effects) Distinguishing Upward and Downward Occupational Mobility

Dependant variable: log(wage)	Short term (0-2)		Long term (3-7)	
	(1)	(2)	(3)	(4)
Job switch	-0.108***	-0.0407***	-0.124***	-0.0374**
	[0.0229]	[0.0155]	[0.0268]	[0.0185]
Within-firm occ. switch UP	0.267***	0.175***	0.264***	0.159***
	[0.0268]	[0.0189]	[0.0324]	[0.0223]
Within-firm occ. switch DOWN	0.113***	0.0655***	0.112**	0.0509*
	[0.0367]	[0.0230]	[0.0438]	[0.0282]
Job-and-occ. switch UP	0.0383	0.0674***	0.00559	0.0227
	[0.0350]	[0.0244]	[0.0389]	[0.0293]
Job-and-occ. switch DOWN	-0.0472	-0.0808***	-0.0341	-0.0635**
	[0.0323]	[0.0220]	[0.0342]	[0.0250]
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes
N	14225	14225	13378	13378
Adj. R-sq	0.024	0.188	0.037	0.134

Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Standard errors clustered at person-level; Observations weighted by length of employment spell; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant.

Table A.8: OLS Regression of Predicted Probabilities of Mobility on the Local Labor Market Conditions at the National Level (Pooling 26 Regions) Accounting for Upward and Downward Mobility

Dependent variable: Predicted probability of	Job switch	Within-firm occ. switch up	Within-firm occ. switch down	Job-and-occ. switch up	Job-and-occ. switch down
	(1)	(2)	(3)	(4)	(5)
Unemployment rate	0.0346***	-0.0157***	0.0120**	0.00549	0.0217***
	[0.0070]	[0.0054]	[0.0053]	[0.0055]	[0.0058]
Unemployment rate^2	-0.00371***	0.00142***	-0.000787	-0.000553	-0.00139**
	[0.0007]	[0.0005]	[0.0005]	[0.0005]	[0.0006]
Unemployment rate^3	0.000114***	-0.0000407**	0.0000281*	0.0000234	0.0000413**
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Unemployment rate < 25 years	0.00565***	-0.0000687	-0.00375***	-0.00142**	-0.00210***
	[0.0008]	[0.0006]	[0.0006]	[0.0007]	[0.0007]
Labor market tightness	-0.00180***	-0.000238	0.00138***	0.000337	0.000914**
	[0.0004]	[0.0003]	[0.0003]	[0.0004]	[0.0004]
Labor market tightness^2	0.0000492***	0.00000598	-0.0000353***	0.0000146*	-0.00000723
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Labor market tightness^3	-0.000000274***	$-4.23e^{-08}$	0.000000208***	-0.000000120***	$-1.35e^{-08}$
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Share low qualified	-0.000840**	-0.000365	0.000276	-0.000348	0.000372
	[0.0004]	[0.0003]	[0.0003]	[0.0003]	[0.0003]
Share highly qualified	0.00295***	-0.000435	0.00128***	0.00110***	0.00321***
	[0.0004]	[0.0003]	[0.0003]	[0.0003]	[0.0004]
Mobility shares					
Unemployment	-0.000214	-0.00155***	-0.00213***	0.000727*	-0.000347
	[0.0005]	[0.0004]	[0.0004]	[0.0004]	[0.0004]
Job switch	-0.00277***	0.00220***	0.00101***	0.000754*	0.00125***
	[0.0005]	[0.0004]	[0.0004]	[0.0004]	[0.0004]
Within-firm occ. switch	0.00236***	-0.000794***	-0.000794***	0.000701***	0.00209***
	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]
Job-and-occ. switch	0.00190***	-0.00287***	-0.000718***	0.000362	-0.000381
	[0.0003]	[0.0002]	[0.0002]	[0.0002]	[0.0002]
Further instrumental variables					
Interaction effects indicating small cells for mobility shares	Yes	Yes	Yes	Yes	Yes
Interaction effects indicating mobility share zero	Yes	Yes	Yes	Yes	Yes
Fixed effects					
Year of graduation	Yes	Yes	Yes	Yes	Yes
2-Digit training occupation	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes
N	14225	14225	14225	14225	14225
Adj. R-sq	0.283	0.123	0.110	0.165	0.199
F-test excl. IVs	25.78	25.87	14.71	8.50	27.87

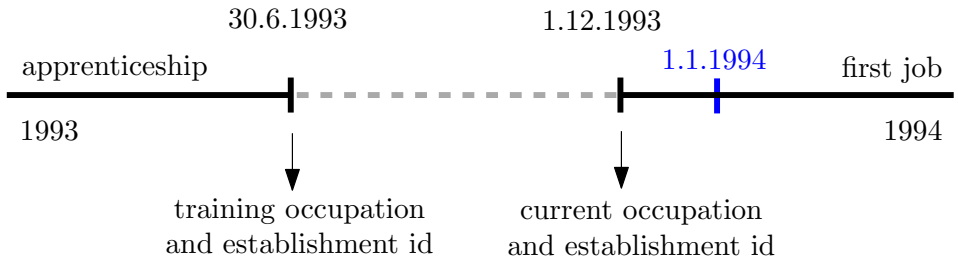
Notes: Standard errors in brackets; * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; Other controls include age at job entrance and dummies for high-school diploma, foreign citizenship, foreign citizenship missing and a constant; Year and year of employment dummies are not required since only one observation per apprenticeship graduate is included.

Table A.9: Key Performance Measures for First Stages of IV Estimates without Heterogeneous Treatment Effects Accounting for Upward and Downward Occupational Mobility

F-Test excl. IVs	Short term (0-2)		Long term (3-7)	
	(1)	(2)	(3)	(4)
Job switch	160.54	301.56	151.51	287.11
Within-firm occ. switch up	84.76	159.83	89.69	177.86
Within-firm occ. switch down	58.51	166.39	55.81	156.63
Job-and-occ. switch up	82.67	150.25	83.42	148.64
Job-and-occ. switch down	89.61	183.95	91.01	183.21
Fixed effects				
Year	Yes	Yes	Yes	Yes
Year of graduation	Yes	Yes	Yes	Yes
Year of employment	Yes	Yes	Yes	Yes
2-Digit training occupation	No	Yes	No	Yes
Other controls	Yes	Yes	Yes	Yes

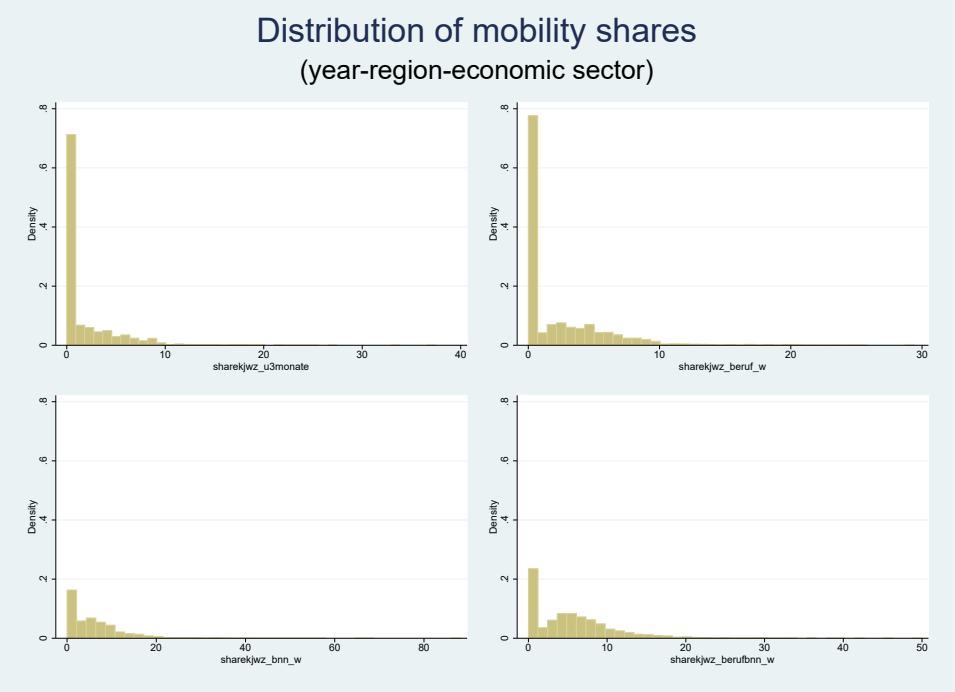
5.1.4 Figure Appendix

Figure A.2: Apprenticeship and First Employment Spell with Interruption



Notes: This example shows the measurement of occupation and establishment ID for an apprentice who graduated in June 1993. His first job held after apprenticeship starts in December 1993 and, thus, lies within the required two-year window after graduation.

Figure A.3: Distribution of Mobility Shares Showing Spikes at Zero for Each of the Four Mobility Groups



Notes: Upper panel, left: mobility share unemployment duration of at least 3 months. Upper panel, right: mobility share within firm occupational switch. Lower panel, left: mobility share job switch. Lower panel, right: mobility share job-and-occupation switch.

Figure A.4: Regional Distribution of Probability Scores for Job Switches (Resulting from Step 1 of IV Procedures, Short Run, Weighted)

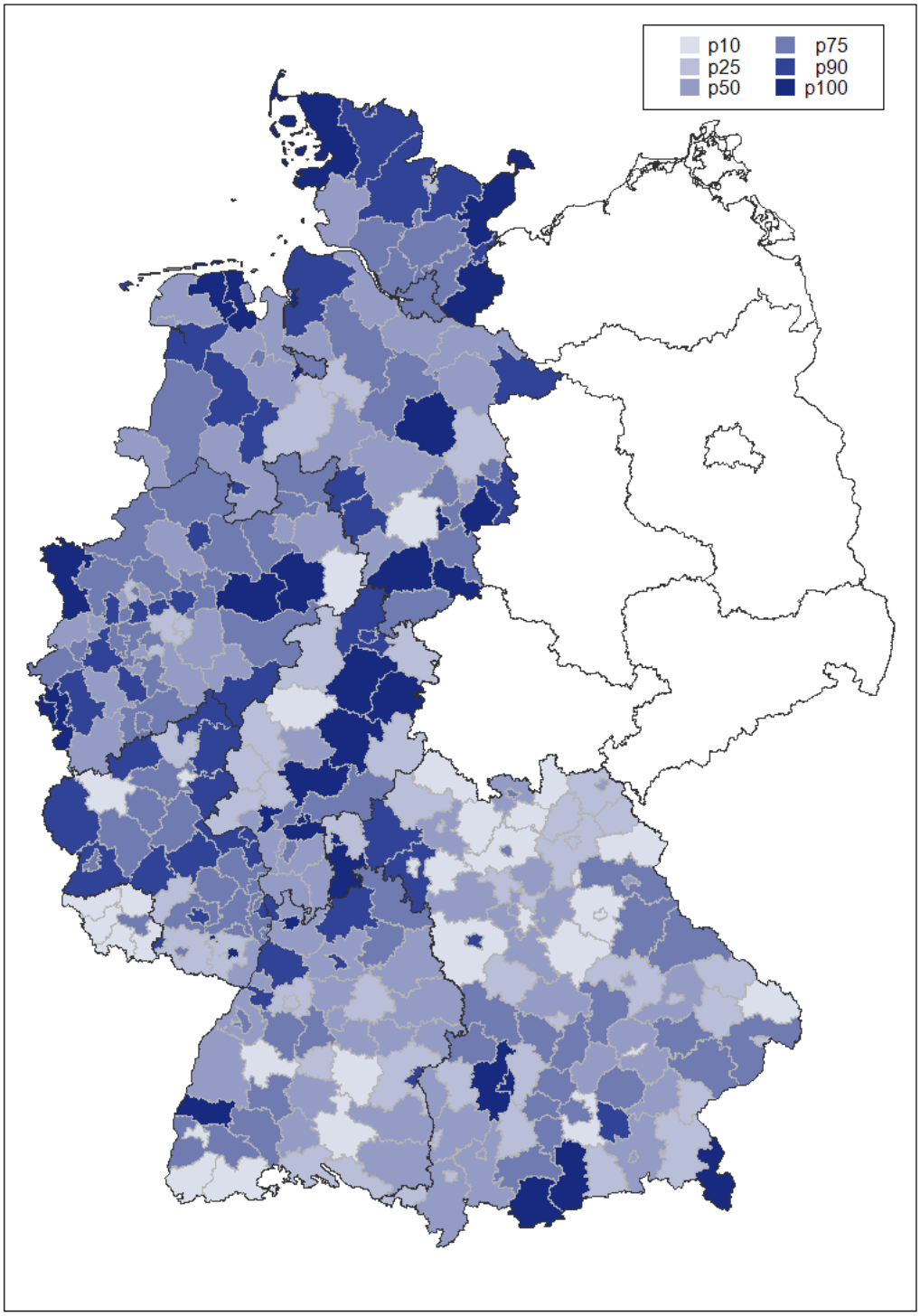


Figure A.5: Regional Distribution of Probability Scores for Within-firm Occupation Switches (Resulting from Step 1 of IV Procedures, Short Run, Weighted)

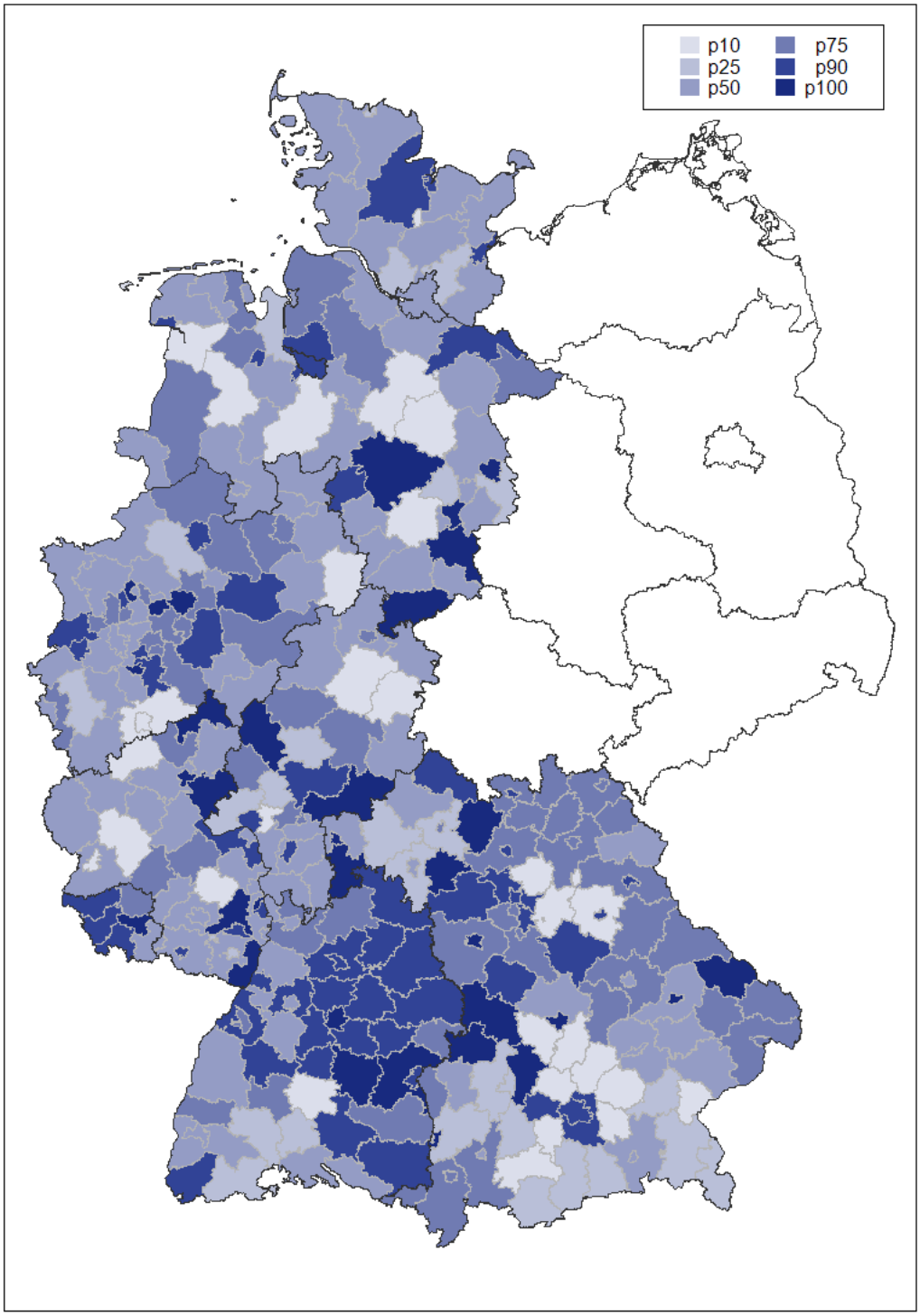


Figure A.6: Regional Distribution of Probability Scores for Job-and-occupation Switches (Resulting from Step 1 of IV Procedures, Short Run, Weighted)

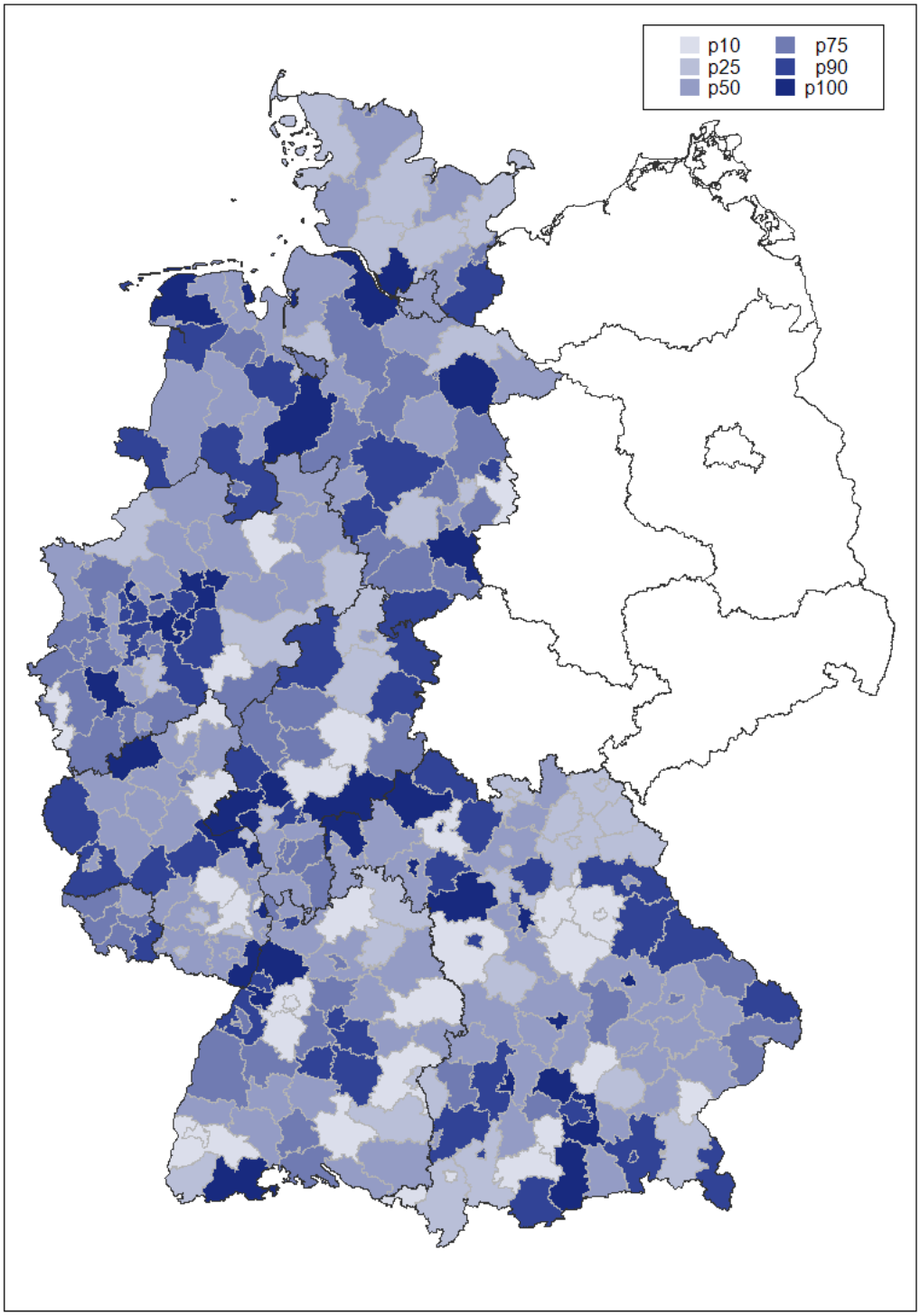
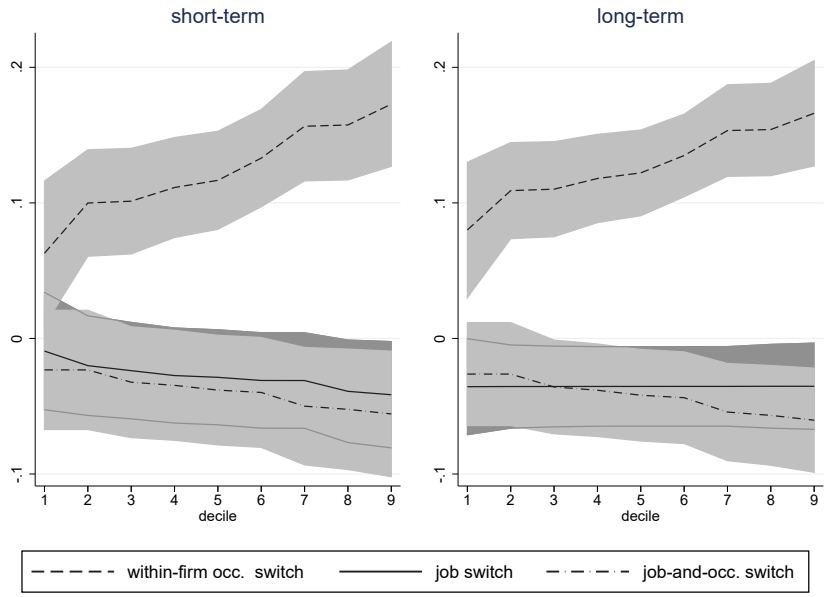
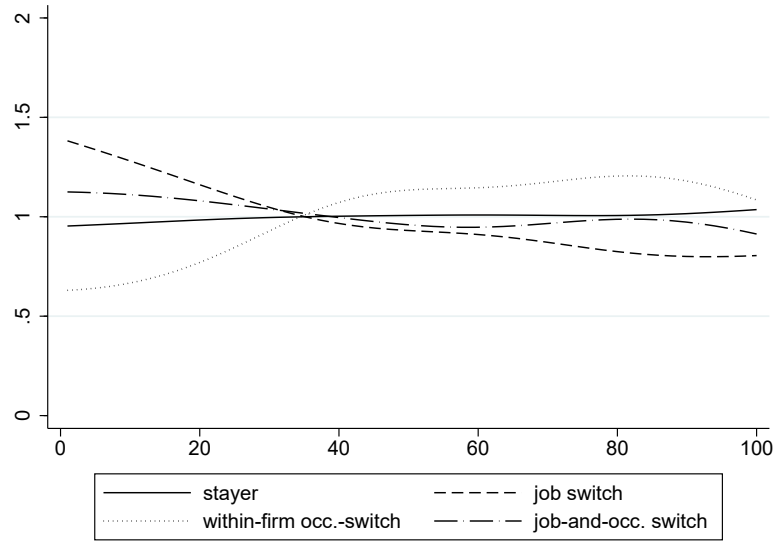


Figure A.7: Average Treatment Effect on the Treated at Deciles of the Group-specific Distribution of Wages in the Training Occupation (Showing 95% Confidence Bands)



Notes: Calculations based on results from 3-step IV estimation controlling for 2-digit training occupations.

Figure A.8: Relative Frequency of Wage Position of Training Occupation by Mobility Group



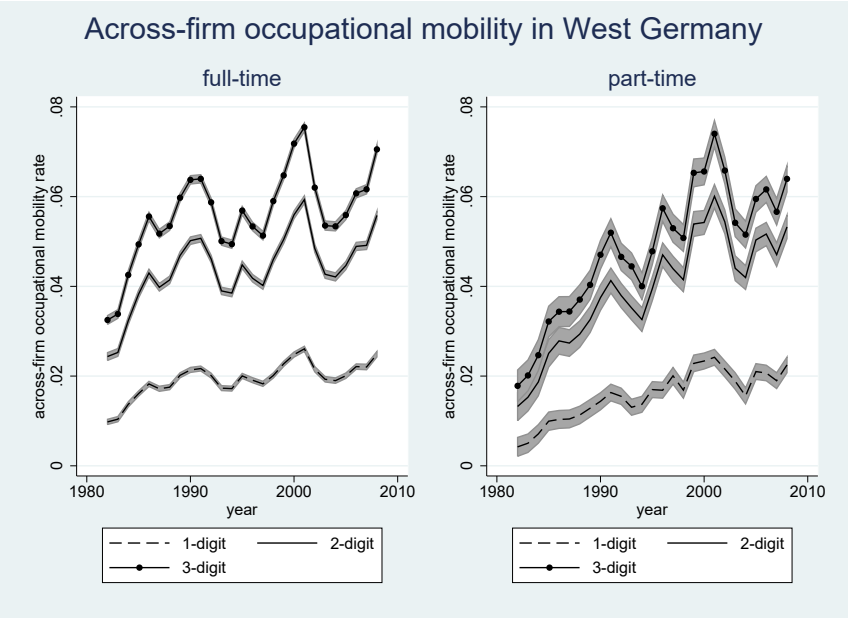
Notes: Occupations ranked from lowest paid (0) to highest paid (100).

5.2 Appendix to Chapter 3: Occupational Mobility in the West German Labor Market

5.2.1 Data Appendix 1: Full-time Versus Part-time Employment

The average share of part-time workers in the main sample is about 8.8% – rising from about 7.1% in 1982 to about 11.0% in 2008. On average over the period 1982–2008 about 92% of part-time workers are female, while women only make up about 26% of full-time employment.

Figure A.9: Across-firm Occupational Mobility at the 3-digit Level by Type of Employment

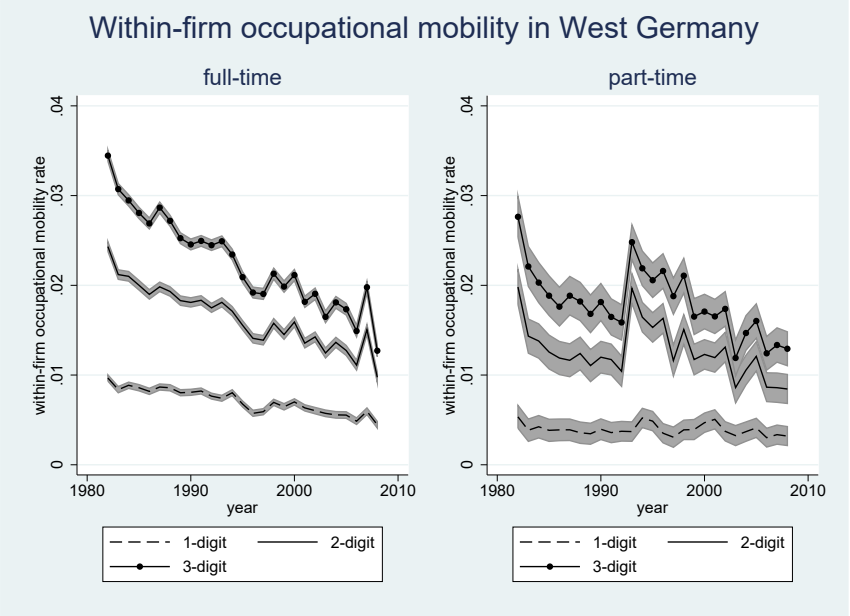


Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figures A.9 and A.10 show the development of occupational mobility rates by type of employment (full-time versus part-time). At the beginning of the observation period across-firm occupational mobility was lower among part-time workers than among full-time workers. However, as Table A.10 shows part-time workers exhibit a steeper linear trend in across-firm occupational mobility than full-time workers and have, in terms of yearly across-firm occupational mobility rates, actually caught up with full-time workers over time. These findings are basically in line with the findings presented in Isaoglu (2010a) based on data from the German Socioeconomic Panel. The author shows that once part-time workers are included in the sample, overall 4-digit occupational mobility rates increase, especially in the second half of the observation period. For occupational mobility within firms Table A.10 reveals a comparably less pronounced negative trend among part-time workers.

As a further robustness check, I exclude all person-year observations in part-time employment from the main sample, and then re-run all analyses on the reduced *full-time sample*. For

Figure A.10: Within-firm Occupational Mobility at the 3-digit Level by Type of Employment



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Table A.10: Trends in Log Mobility Rates over the Period 1982–2008 in West Germany by Type of Employment

	occupational mobility across firms	occupational mobility within firms	mobility across firms
	A. Main analysis (full sample)		
1-digit	0.0222***	-0.0233***	
2-digit	0.0189***	-0.0246***	
3-digit	0.0170***	-0.0276***	
			0.0180***
	B. Full-time employment only		
1-digit	0.0208***	-0.0235***	
2-digit	0.0173***	-0.0249***	
3-digit	0.0156***	-0.0280***	
			0.0169***
	C. Part-time employment only		
1-digit	0.0481 ***	-0.0062	
2-digit	0.0423 ***	-0.0154 ***	
3-digit	0.0389 ***	-0.0175 ***	
			0.0335 ***

Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of log(yearly mobility rates). Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

the sample of full-time workers only, the positive trend in across-firm occupational mobility is less pronounced, but statistically significant, at all digit levels. Apart from this, the main results of the paper presented above are largely robust to restricting the sample to full-time workers.

5.2.2 Table Appendix

Table A.11: Classification of Career Episodes

type (ID)	description
No intermittent employment in East German establishment:	
0	employment in West Germany without interm. unemployment
1	employment in West Germany with interm. unemployment less than 365 days
4	employment with total of intermittent days in unemployment >= 365 days
5	employment with total of intermittent days missing from data set >= 365 days
Intermittent employment in East German establishment:	
6	employment in West Germany with interm. employment in East Germany
7	employment in East Germany (also on 30th of June) leading up to employment in West Germany
8	employment with total of intermittent days in unemployment >= 1 year
9	employment with total of intermittent days missing from data set >= 1 year

Note: Berlin counts as East Germany.

Table A.12: Overview of 1-digit and 2-digit Classifications of Occupations Used in the Analysis

ID	description
	A. List of 1-digit occupations:
1	Agricultural occupations
3	Occupations in manufacturing; occupations in mining
4	Technical occupations
5	Service Occupations
	B. List of 2-digit occupations:
1	Farmers, animal breeders, occupations in fishing
2	Miners, mineral extractors, occupations in processing of stones, manufacturers of building materials
4	Ceramics workers, glass makers
5	Chemical workers, plastics processors
6	Paper makers, products makers, printers
7	Wood preparers, wood product makers and related occupations
8	Metal producers, workers
9	Locksmiths, mechanics and assigned occupations
10	Electricians
11	Assemblers and occupations in metal n.e.c.
12	Occupations in textile- and leather-making and -processing
14	Occupations in food-production and -processing
15	Occupations in building construction
16	Building-, interior decorator, upholsterers; carpenters, model makers
18	Painters, lacquerers and related occupations
19	Goods examiners, dispatchers
20	Assistants (no further specification)
21	Machinists and related occupations
22	Engineers, chemists, physicists, mathematicians
23	Technicians, technical specialists
24	Management assistants in trade, other sales personnel
25	Bankers, insurance salespersons, management assistants in logistics, real estate agents and related occupations
26	Occupations in traffic
27	Occupations in organization, public administration, office
28	Occupations in safety and security
29	Occupations in editorial work and journalism, in artisan craft work and fine arts, in the performing arts and entertainment
30	Occupations in health care
31	Occupations in social services and education, n.e.c. occupations in humanities and natural sciences
32	General service occupations

Table A.13: Share of Missings in the Occupation Variable (%) for the Period 1982–2008

	raw data (employment spells)	person-year-panel	basic sampling rules	full sampling rules
share in 1982	0.25	0.21	0.13	0.04
share in 2008	1.93	1.24	1.01	0.40
average share	1.68	0.57	0.44	0.17

Note: Full sampling rules still exclude all sampling rules related to missings in occupation variable.

Table A.14: Comparison of Linear and Quadratic Trends in Log Mobility Rates over the Period 1982–2008 in West Germany.

		occupational mobility across firms	occupational mobility within firms	mobility across firms
		A. Linear trend (main analysis)		
1-digit		0.0222***	-0.0233***	
2-digit		0.0189***	-0.0246***	
3-digit		0.0170***	-0.0276***	
				0.0180***
		B. Quadratic trend		
1-digit	t	0.0705***	-0.0143*	
	t^2	-0.0017***	-0.0003	
2-digit	t	0.0605***	- 0.0223**	
	t^2	-0.0015***	-0.001	
3-digit	t	0.0564***	-0.0273***	
	t^2	-0.0014***	-0.00001	
	t			0.0534***
	t^2			-0.0013***

Estimation results from OLS estimations with a constant. Regressions performed on time series of log(yearly mobility rates). Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

Table A.15: Average Yearly Occupational Mobility Rates (%) at the 3-digit Level over the Period 1982–2008 in West Germany by Gender, Education, and Age

	Male workers across firms	Male workers within firms	Female workers across firms	Female workers within firms
	A. No degree or only highschool degree			
20-24 years	17.05	3.09	13.16	2.30
25-34 years	7.80	2.59	6.91	2.33
35-44 years	3.46	2.37	4.07	1.78
45-54 years	1.86	2.30	2.20	1.66
55-60 years	0.90	1.91	0.85	1.43
	B. Vocational training degree (with or without highschool degree)			
20-24 years	14.24	2.86	10.27	1.87
25-34 years	9.01	2.75	7.37	1.93
35-44 years	4.87	2.39	4.80	1.88
45-54 years	3.00	2.14	2.97	1.73
55-60 years	1.64	1.87	1.55	1.76
	C. Technical college or university degree			
20-24 years	21.80	4.47	16.88	2.43
25-34 years	10.82	2.68	11.18	2.38
35-44 years	5.88	2.33	5.74	2.21
45-54 years	3.48	2.00	3.33	1.76
55-60 years	2.05	1.88	1.85	1.45

Table A.16: Trends in Log Across-firm Mobility Rates over the Period 1982–2008 in West Germany by Gender, Education, and Age

	Male workers			Female workers		
	1-digit	2-digit	3-digit	1-digit	2-digit	3-digit
	A. No degree or only high school degree					
20-24 years	0.0534***	0.0409***	0.0389***	0.0542***	0.0489***	0.0468***
25-34 years	0.0541***	0.0441***	0.0439***	0.0559***	0.0517***	0.0513***
35-44 years	0.0556***	0.0442***	0.0418***	0.0499***	0.0409***	0.0428***
45-54 years	0.0386***	0.035***	0.0302***	0.0375***	0.0364***	0.0378***
55-60 years	0.0311***	0.0361***	0.0309***	0.034*	0.0355***	0.0425***
	B. Vocational training degree (with or without high school degree)					
20-24 years	0.0242***	0.016***	0.0136***	0.0145**	0.0209***	0.0147***
25-34 years	0.0227***	0.0199***	0.0179***	0.0282***	0.0281***	0.0212***
35-44 years	0.0233***	0.0171***	0.0139***	0.0303***	0.026***	0.019***
45-54 years	0.0241***	0.0202***	0.0164***	0.0282***	0.0234***	0.0177***
55-60 years	0.0417***	0.0332***	0.0306***	0.0266***	0.0288***	0.029***
	C. Technical college or university degree					
20-24 years	0.0374***	0.0255**	0.0294***	0.029**	0.0279***	0.0307***
25-34 years	0.037***	0.0325***	0.0325***	0.0268***	0.0301***	0.0246***
35-44 years	0.0171***	0.0185***	0.0202***	0.029**	0.0269***	0.0163**
45-54 years	0.0145**	0.0143***	0.0184***	0.0267**	0.0442***	0.028***
55-60 years	0.0342***	0.0404***	0.0463***	0.02	-0.0098	0.0188

Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of yearly mobility rates. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

Table A.17: Trends in Log Within-firm Mobility Rates over the Period 1982–2008 in West Germany by Gender, Education, and Age

	Male workers			Female workers		
	1-digit	2-digit	3-digit	1-digit	2-digit	3-digit
	A. No degree or only high school degree					
20-24 years	-0.0241**	-0.0361***	-0.0383***	-0.0091	-0.0174**	-0.0203***
25-34 years	-0.0293***	-0.0321***	-0.0312***	-0.0056	-0.0215***	-0.0197***
35-44 years	-0.0267***	-0.027***	-0.0305***	-0.0224***	-0.0284***	-0.0285***
45-54 years	-0.0392***	-0.0334***	-0.0371***	-0.0328***	-0.0325***	-0.0341***
55-60 years	-0.0564***	-0.0416***	-0.0394***	-0.0347***	-0.0314***	-0.0302***
	B. Vocational training degree (with or without high school degree)					
20-24 years	-0.0167***	-0.0188***	-0.0232***	-0.0136**	-0.0164***	-0.0279***
25-34 years	-0.0205***	-0.0209***	-0.0249***	-0.0154***	-0.0202***	-0.0291***
35-44 years	-0.0173***	-0.0165***	-0.0216***	-0.0181***	-0.0247***	-0.0303***
45-54 years	-0.0241***	-0.0212***	-0.0238***	-0.0268***	-0.0357***	-0.0388***
55-60 years	-0.0335***	-0.0345***	-0.0353***	-0.0257***	-0.04***	-0.0425***
	C. Technical college or university degree					
20-24 years	0.002	-0.0106	-0.0269*	-0.0235***	-0.0216	-0.0284**
25-34 years	-0.0149***	-0.0157***	-0.0175***	-0.0312***	-0.0182***	-0.0178***
35-44 years	-0.0263***	-0.0267***	-0.0275***	-0.0409***	-0.0318***	-0.0274***
45-54 years	-0.0244***	-0.0239***	-0.0236***	-0.0395**	-0.0155	-0.0238**
55-60 years	-0.0299***	-0.0323***	-0.0274***	-0.0302	-0.0607***	-0.0477***

Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of yearly mobility rates. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

Table A.18: Trends in Log Firm-to-firm Mobility Rates over the Period 1982–2008 in West Germany by Gender, Education, and Age

	Male workers	Female workers
	A. No degree or only high school degree	
20 - 24 years	0.0397***	0.0459***
25-34 years	0.0432***	0.0489***
35-44 years	0.0423***	0.0406***
45-54 years	0.0357***	0.037***
55-60 years	0.0349***	0.0328***
	B. Vocational training degree (with or without high school degree)	
20 - 24 years	0.0106***	0.0165***
25-34 years	0.0161***	0.0227***
35-44 years	0.0134***	0.0205***
45-54 years	0.0175***	0.0191***
55-60 years	0.029***	0.0232***
	C. Technical college or university degree	
20 - 24 years	0.0213***	0.0162***
25-34 years	0.0254***	0.0181***
35-44 years	0.0199***	0.0137***
45-54 years	0.0224***	0.0373***
55-60 years	0.0328***	0.0497***

Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of yearly mobility rates. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

Table A.19: Trends in Log Mobility Rates (%) by Type of Career Episode over the Period 1982–2008 in West Germany

	occupational mobility across firms	occupational mobility within firms	mobility across firms
A. Employment in West Germany without interm. unemployment			
1-digit	0.0220***	-0.0243***	
2-digit	0.0198***	-0.0256***	
3-digit	0.0181***	-0.0285***	
			0.0214***
B. Employment in West Germany with interm. unemployment			
1-digit	0.0198***	-0.0057	
2-digit	0.0150***	-0.0092**	
3-digit	0.0133***	-0.0142***	
			0.0102***
C. Intermittent days in unemployment >= 365 days			
1-digit	0.0077**	-0.0371*	
2-digit	0.0062***	-0.0323**	
3-digit	0.0040***	-0.0306**	
			0.0019***
D. Intermittent days missing from data set >= 365 days			
1-digit	0.0258***	-0.0125***	
2-digit	0.0209***	-0.0199***	
3-digit	0.0189***	-0.0230***	
			0.0147***

Estimation results from OLS estimations with trend variable and constant. Regressions performed on time series of log(yearly mobility rates). Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Estimated coefficients need to be interpreted as semi-elasticities.

5.2.3 Figure Appendix

Figure A.11: Share of Missings in the Occupation Variable

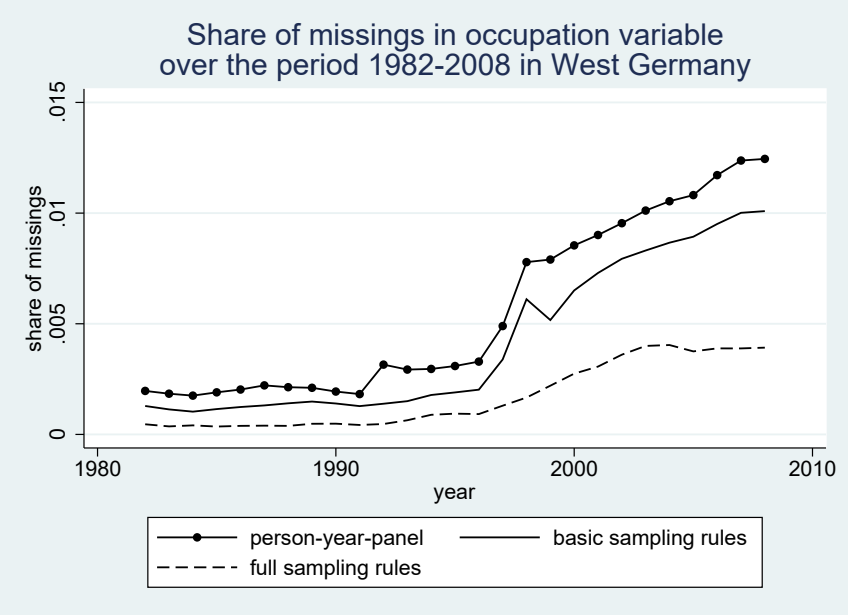
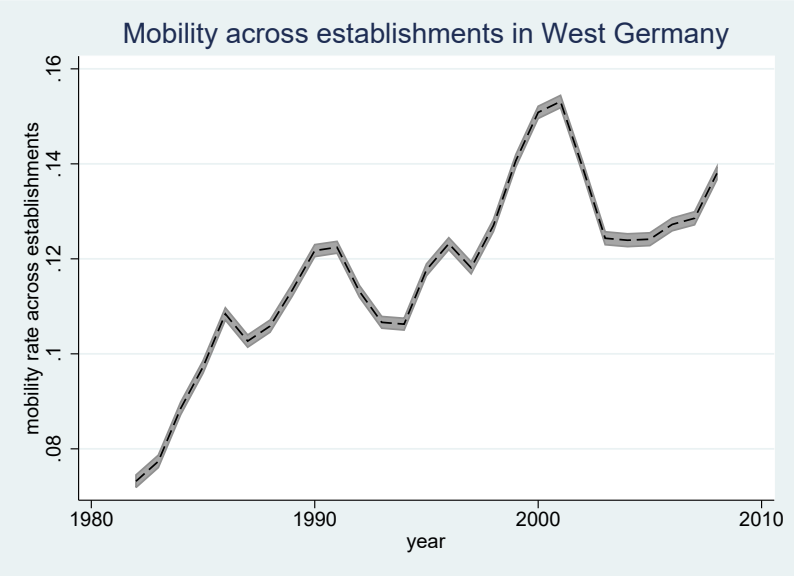


Figure A.12: Mobility across Establishments over the Period 1982–2008 in West Germany



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure A.13: Across-firm Occupational Mobility at the 3-digit Level and GDP Growth

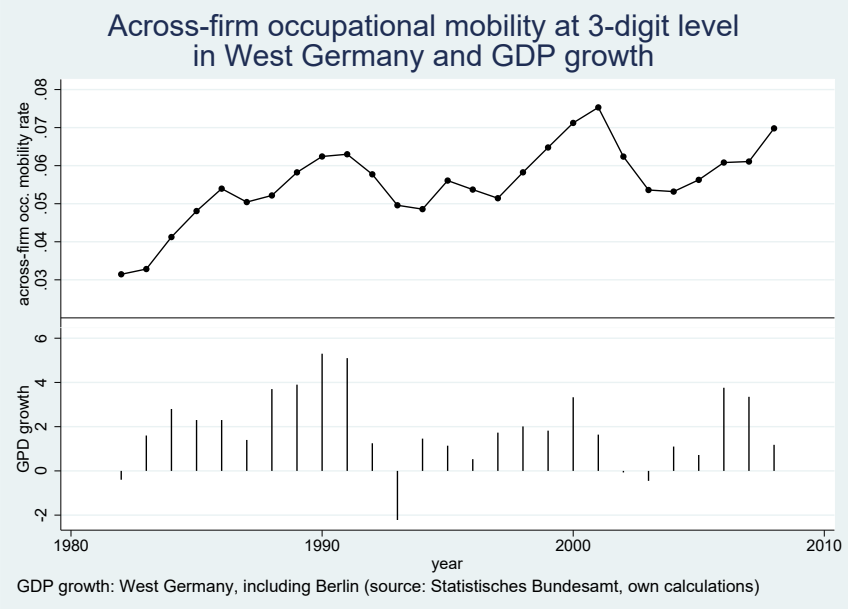


Figure A.14: Mobility Across Establishments and GDP Growth

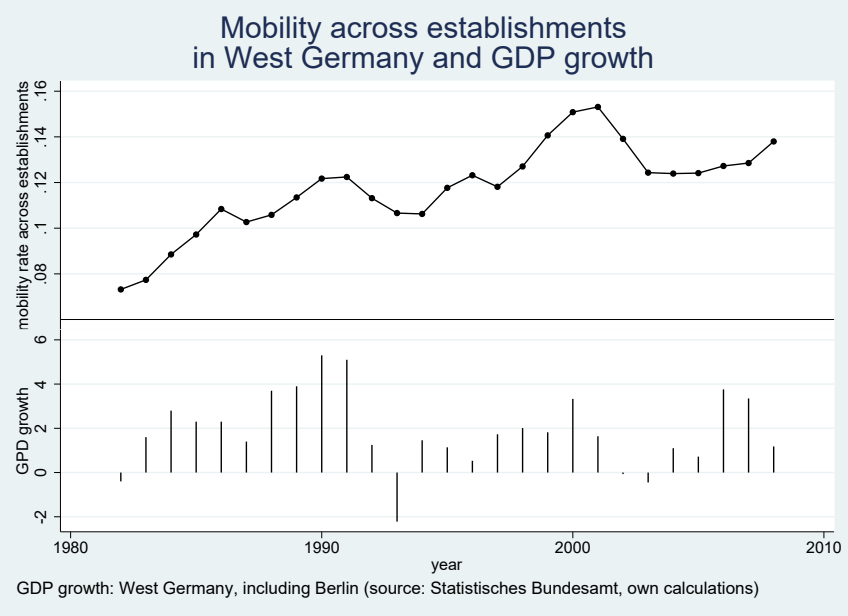


Figure A.15: Within-firm Occupational Mobility at the 3-digit Level and GDP Growth

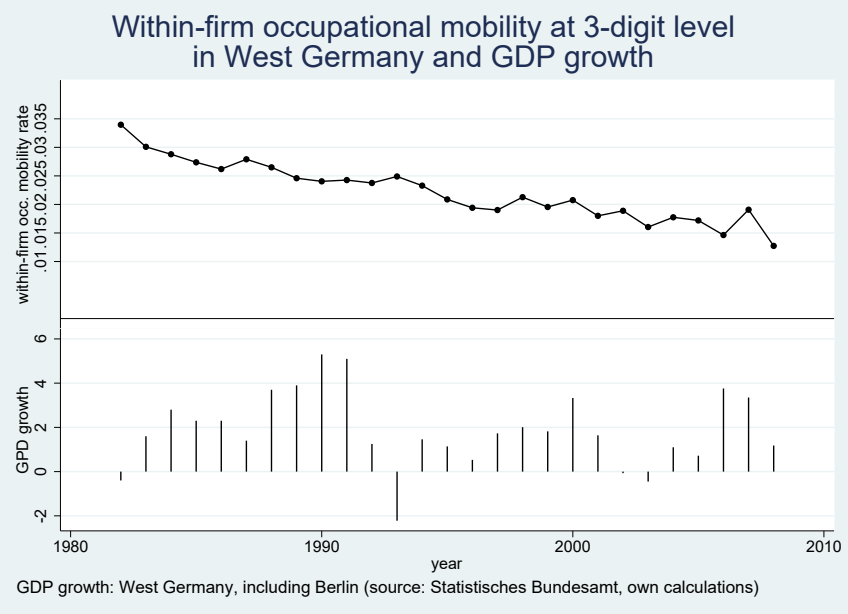
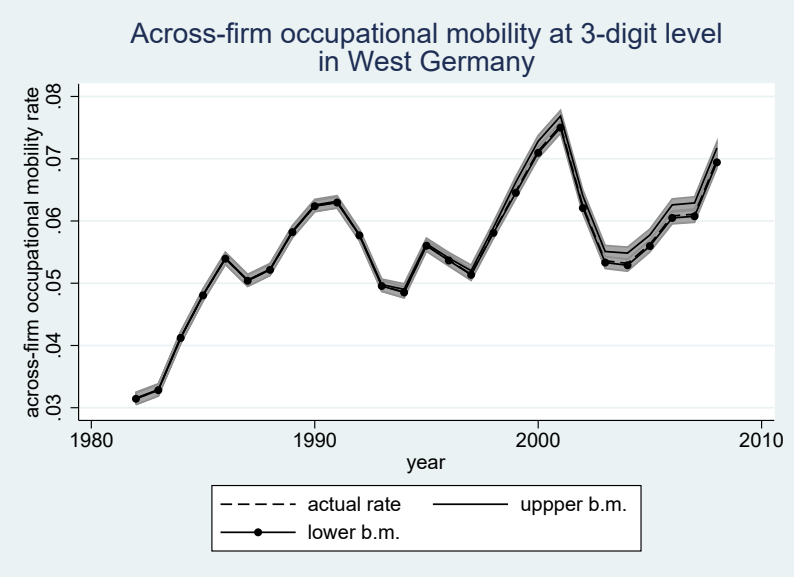
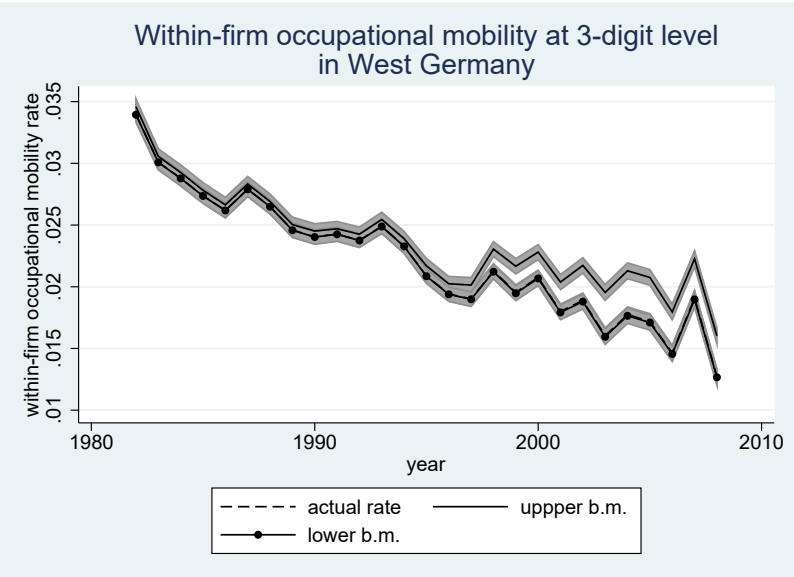


Figure A.16: Robustness Check: Across-firm Occupational Mobility at the 3-digit Level when Making Different Assumptions on the Meaning of Missings in the Occupation Variable



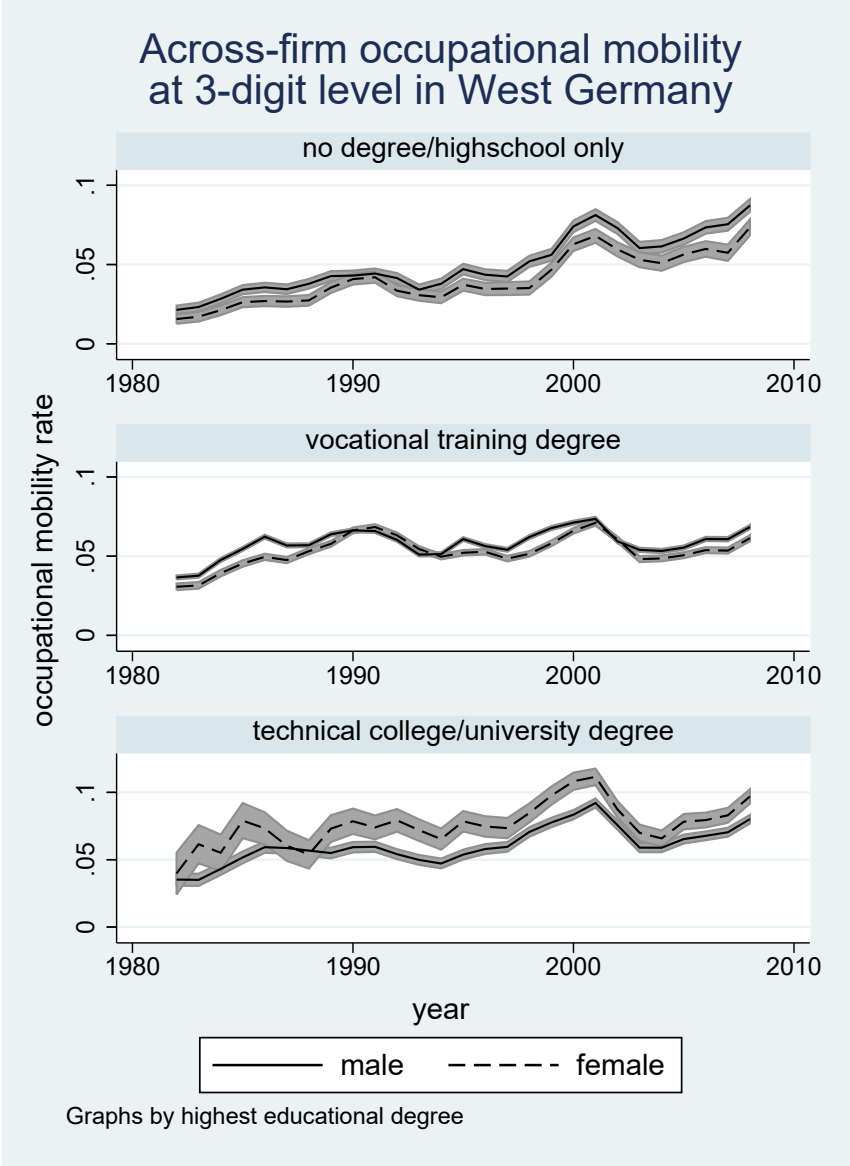
Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure A.17: Robustness Check: Within-firm Occupational Mobility at the 3-digit Level when Making Different Assumptions on the Meaning of Missings in the Occupation Variable



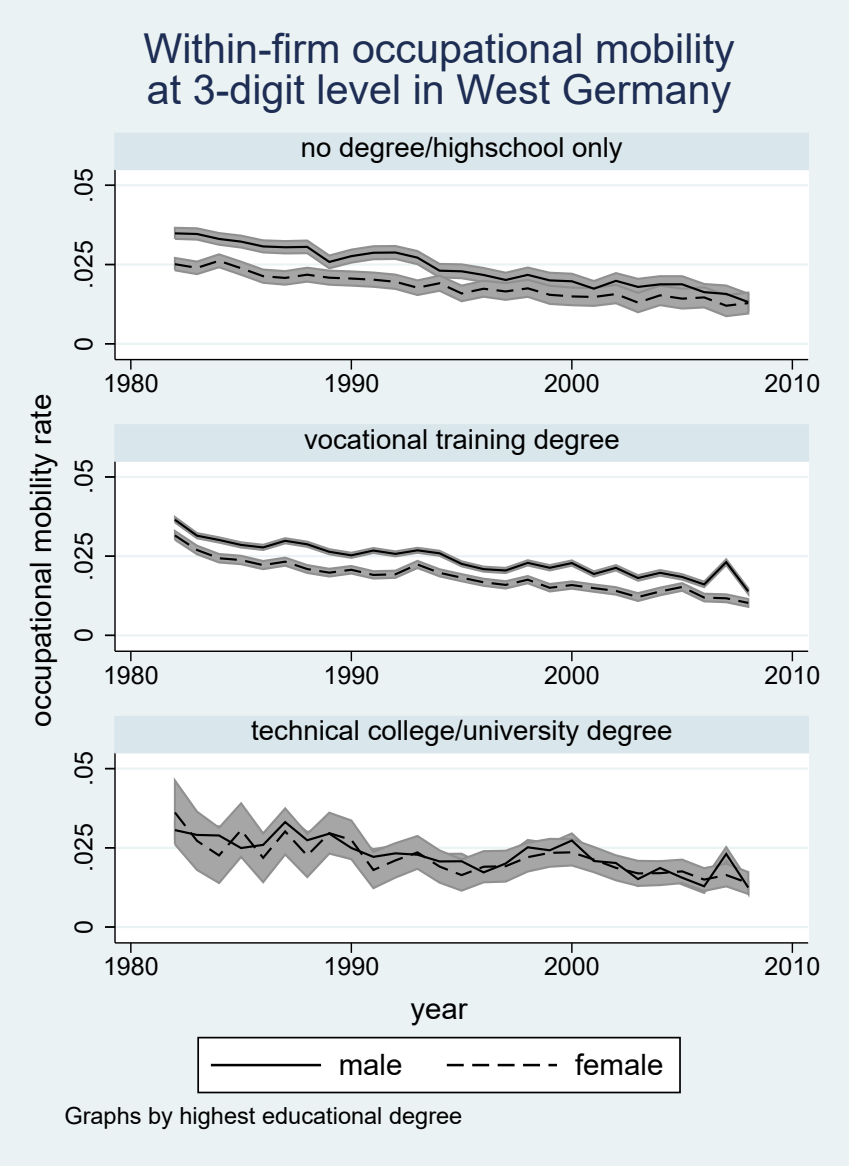
Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.
Lines for actual rate and lower benchmark fully overlap.

Figure A.18: Across-firm Occupational Mobility at the 3-digit Level by Highest Educational Degree and Gender



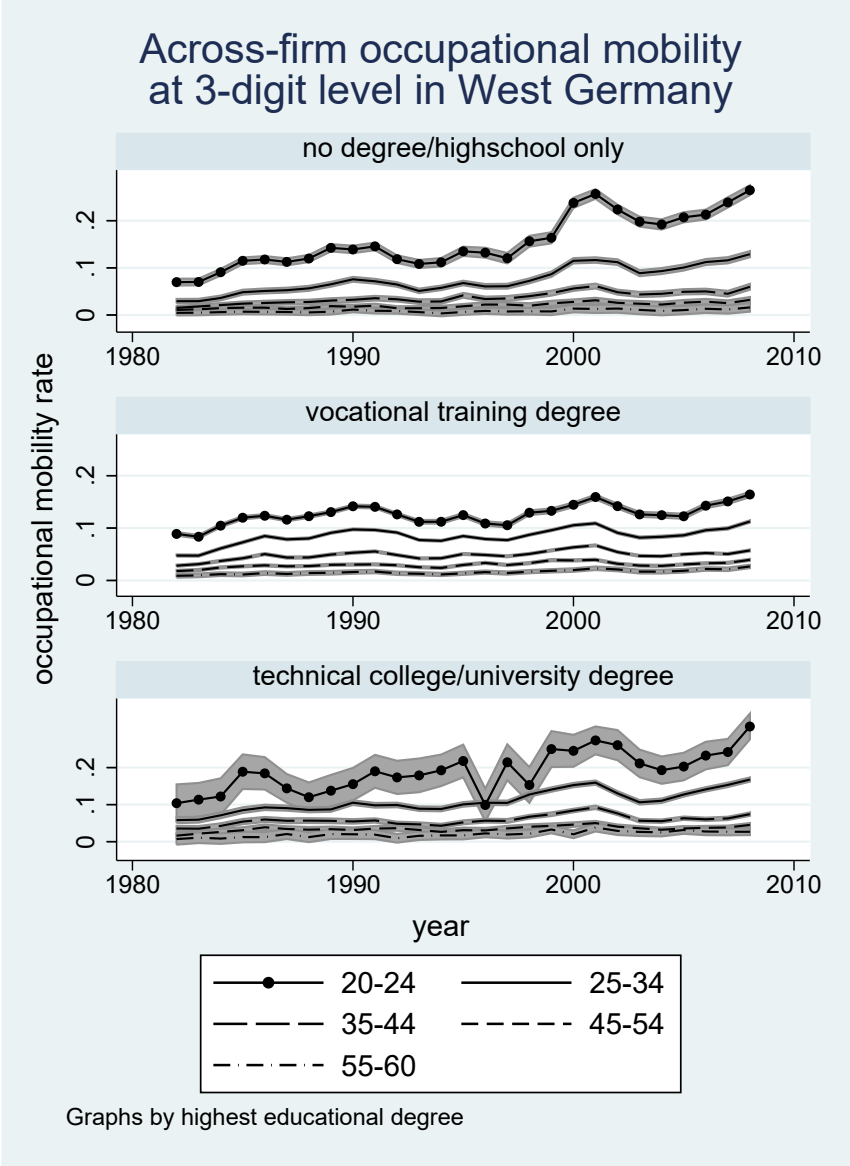
Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure A.19: Within-firm Occupational Mobility at the 3-digit Level by Highest Educational Degree and Gender



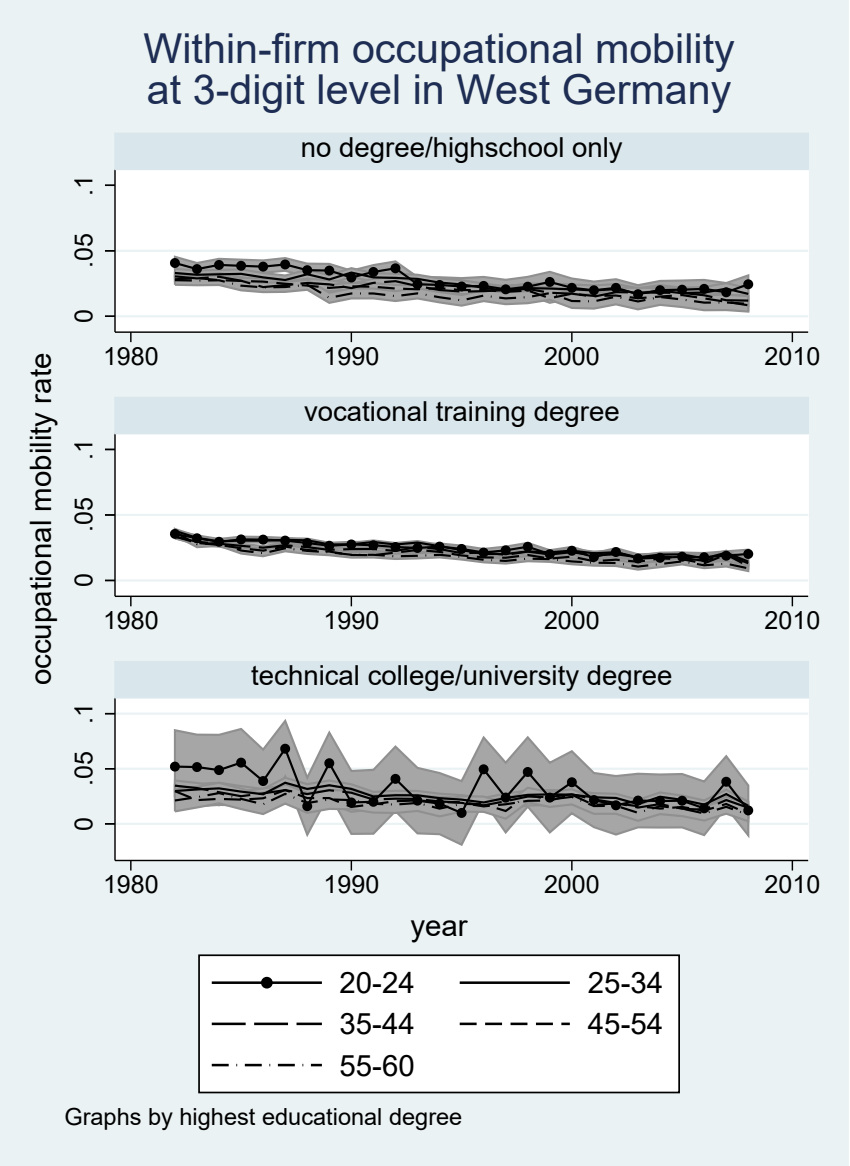
Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure A.20: Across-firm Occupational Mobility at the 3-digit Level by Highest Educational Degree and Age Groups



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure A.21: Within-firm Occupational Mobility at the 3-digit Level by Highest Educational Degree and Age Groups



Note: Mobility rate estimates obtained from OLS regression, showing 95% confidence bands.

Figure A.22: Net Occupational Mobility over the Period 1982–2008 in West Germany

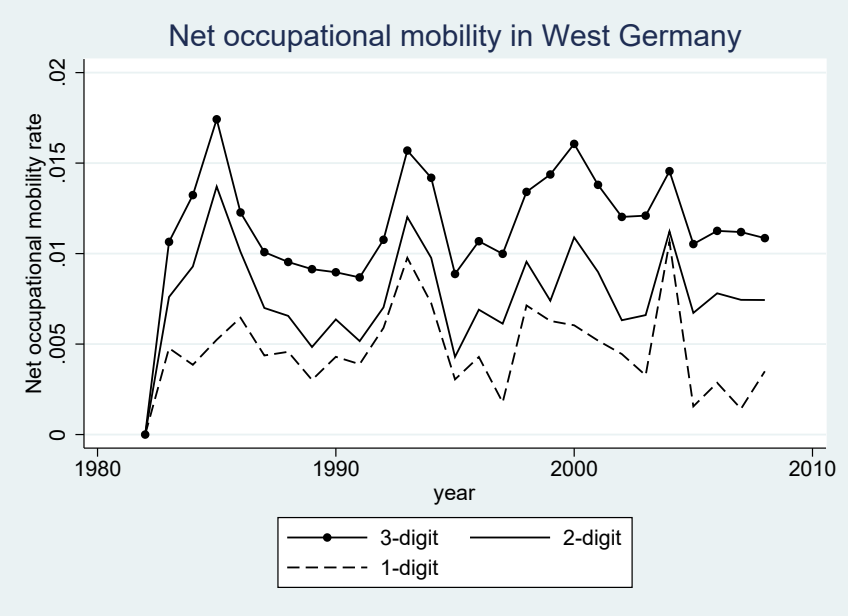


Figure A.23: Across-firm Occupational Mobility at the 3-digit Level and Share of Upward Mobility by Type of Career Episode

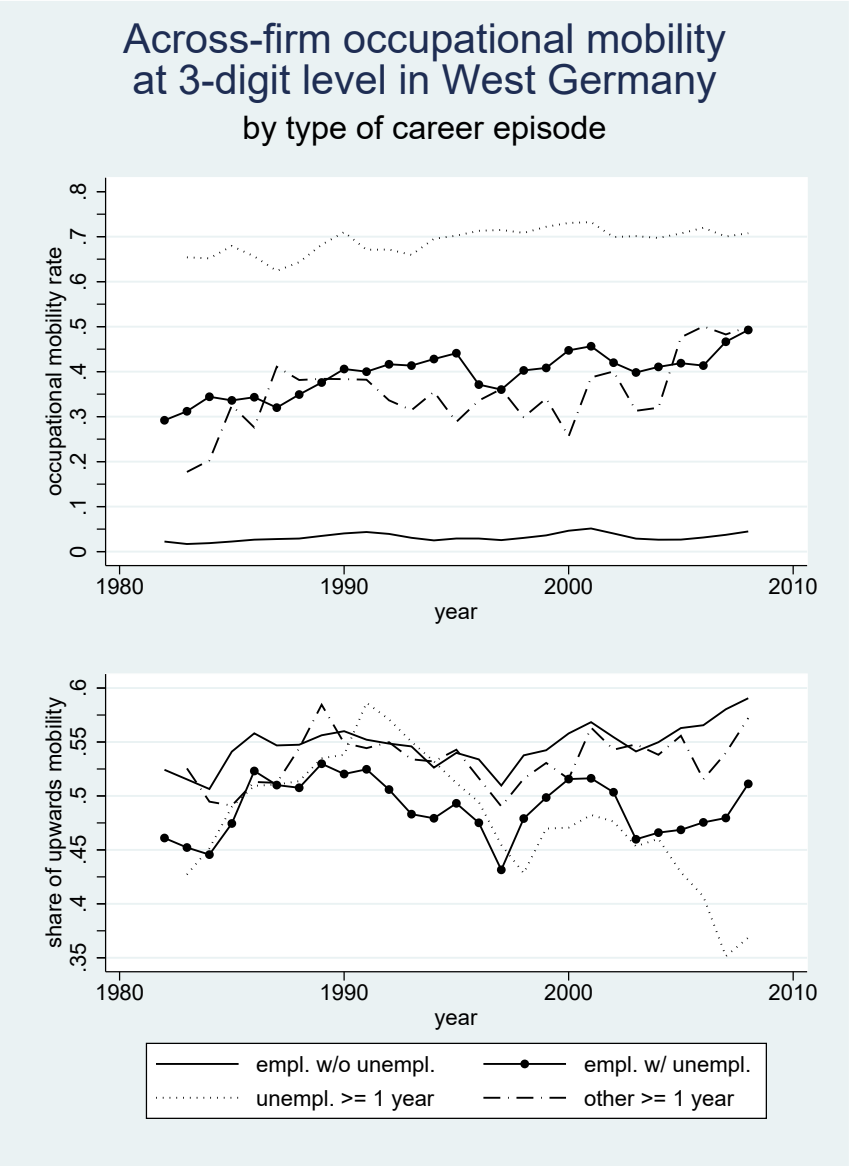
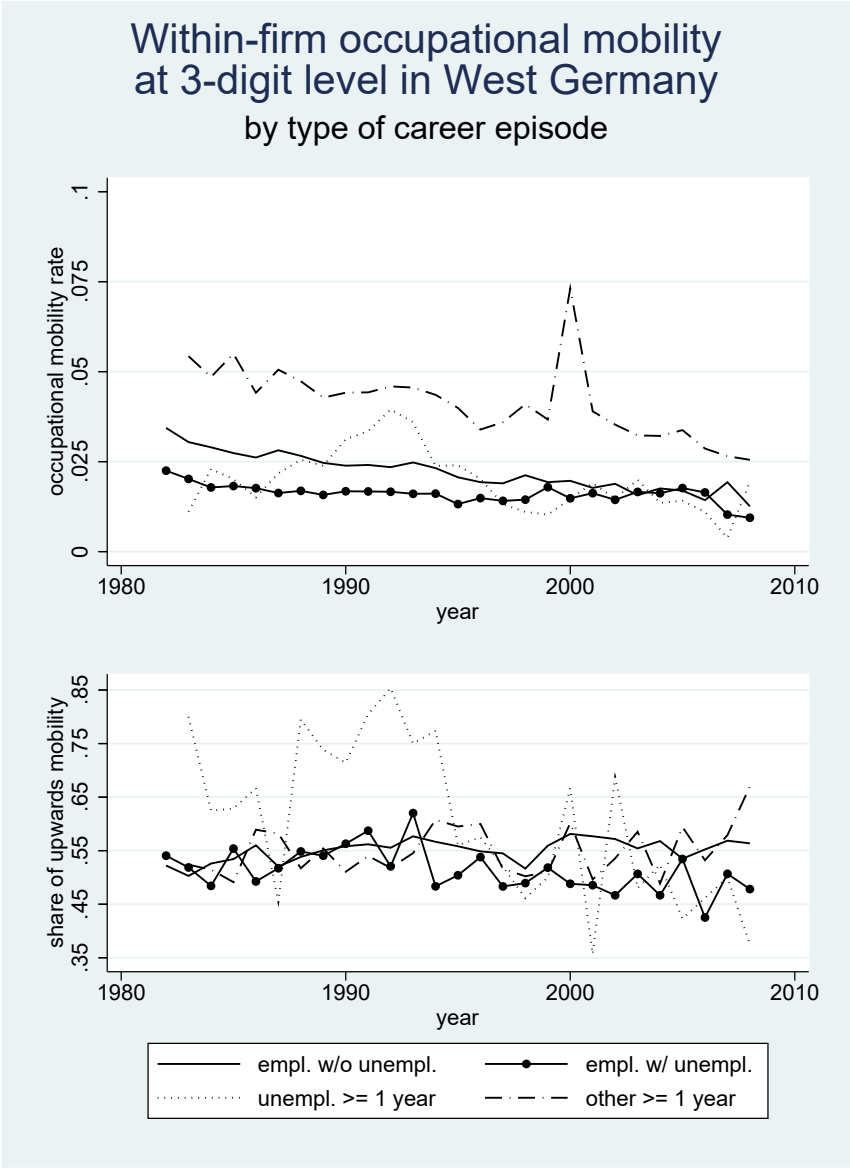


Figure A.24: Within-firm Occupational Mobility at the 3-digit Level and Share of Upward Mobility by Type of Career Episode



5.3 Appendix to Chapter 4: Product Market Deregulation and Labor Market Outcomes in the German Skilled Crafts and Trades

5.3.1 Data Appendix 1: Technical Details on Analysis of Self-employment

In Section 4.4.3, we study the link between product market deregulation and the total number of self-employed, as well as on a wide range of self-employment probabilities. All results on self-employment are based on survey data from the Microcensus SUF 2000–2008 provided by the Research Data Centers of the German Federal Statistical Office and the Statistical Offices of the German Federal States. Table A.20 describes all samples used in our analysis of self-employment. Our differences-in-differences analysis of the total number of self-employed (head count) is based on an occupations panel. For the respective first-differences estimations, we then compute averages over the pre-reform and post-reform period, respectively, to create the required two-period panel. The other outcomes are analyzed at the individual level. The estimations for the probability of being self-employed among all employed craftsmen, which should be strongly related to the self-employment share, are based on a sample comprising all self-employed and employed craftsmen (*self-employment share sample*). The effects on the probability of being self-employed are based on the *base sample*, which contains all craftsmen who are currently employed, unemployed, or out of the labor force. For our analysis of the probability of entry into self-employment we use the *entry sample*. The effects on the exit probabilities are estimated using the *self-employment exit sample* that only contains craftsmen who were self-employed 12 months ago. In our analysis of the probability of being self-employed, as well as of entry and exit probabilities, the definition of the dependent variables and the samples chosen are similar to those in Rostam-Afschar (2014). Lastly, the estimation sample for the probability of being newly self-employed among all self-employed craftsmen consists of currently self-employed craftsmen only. Runst *et al.* (2016) analyze two alternative measures of entry into self-employment, the second of which is very similar to our new self-employment outcome variable. Table A.21 defines the most important variables used in our analysis: outcome variables, covariates, and variables used in the robustness checks.

Table A.20: Overview of All Samples Used in the Analysis of Self-employment Based on Survey Data (Microcensus)

Individual-level data:	
Base	**scope:** all self-employed, dependently employed, unemployed, or out of the labor force during the period 2000–2008 with reported occupation belonging to control or treatment occupations **sampling criteria:** male and female, aged 25–55, no institutionalized persons, no missing information in educational variable, no university or technical college degree; no civil servants, soldiers, apprentices, persons in military service or alternative civilian service, or persons helping in the family business **number of observations:** about 195,000 observations in 23 control and 31 treatment occupations (about 43% of observations in treatment group)
Entry	**scope:** self-employed, dependently employed, unemployed, or out of the labor force **additional sampling criteria:** only individuals with non-missing information on their labor market status and (if applicable) type of employment 12 months ago*
New self-employment	**scope:** only currently self-employed individuals **additional sampling criteria:** only individuals with non-missing information regarding the year in which the current professional activity was started
Self-employment (exit)	**scope:** self-employed, dependently employed, unemployed, or out of the labor force, who report that they were self-employed 12 months ago **additional sampling criteria:** only individuals with non-missing information on their labor market status and type of employment 12 months ago*
Self-employment (share)	**scope:** only currently self-employed and dependently employed individuals **additional sampling criteria:** none
Occupation-level data:	
Occupation-year panel	obtained by aggregating individual-level data by year and occupation

Note: * Questions regarding labor market activities 12 months before the interview were only posed to a 0.45% subsample of the population in the survey waves 2000–2004, and answering them was never mandatory.
"Additional sampling criteria" mentions all sampling criteria implemented on top of the base sampling criteria.

Table A.21: Definition of Important Variables Used in the Analysis of Self-employment Based on Survey Data (Microcensus)

Variable	Description
Individual-level variables:	
Self-employment	A dummy variable equal to 1 if the person is currently working and reports being self-employed with/without employees.
Entry into self-employment	A dummy variable equal to 1 if the person is currently working and reports being self-employed with/without employees, but was not self-employed with/without employees (either not working or dependently employed) 12 months ago. The latter condition is constructed using information on the individual's labor market activities 12 months ago. In the survey waves 2000–2004, this information was only collected for a subsample of 0.45% of the population, and answering the respective question wasn't mandatory over the whole period 2000–2008. With about 50% the share of missings is high, but they are proportionally distributed over treatment and control group.
Exit out of self-employment	A dummy variable equal to 1 if the person is currently not self-employed with/without employees (either not working or dependently employed), but was reported as working and being self-employed with/without employees 12 months ago. The latter condition is also constructed using information on the individual's labor market activities 12 months ago (see details above).
New self-employment	A dummy variable equal to 1 if the person is currently working and reports being self-employed with/without employees, and the current job was started in the year in which the survey was taken. The latter condition is constructed using information on the starting year of the current job, which exhibits a share of missings of about 1.7%. Missings are proportionally distributed over treatment and control group.
No vocational training	A dummy variable equal to 1 if the person does not hold a vocational training degree.
Vocational training	A dummy variable equal to 1 if the person does at least hold a vocational training degree (including persons holding a master craftsman certificate).
Master craftsman degree	A dummy variable equal to 1 if the person holds a master craftsman certificate.
East EU	A dummy variable equal to 1 if the self-employed is a citizen of one of the following new member countries of the EU expansion to the East in 2004: Czech Republic, Hungary, Poland, Slovakia. These are the countries which we can identify individually. According to Müller (2006), p. 68 the by far largest influx of new crafts entrepreneurs into Germany came from Poland.
Subsidies	A dummy variable equal to 1 if the self-employed (i) receives public transfers starting in 2003 or sometime after 2003, which are not child benefits (no eligible children in household), (ii) pays contributions to statutory pension insurance ("GRV"), (iii) earns less than about 25,000 Euros per year, (iv) and has started his current job no more than 3 years ago. Criteria (ii)–(iv) mimic the specific rules applying to recipients of the start-up subsidy Me Inc. as closely as possible. The variable is a modified version of the subsidies variable used in Rostam-Afschar (2014).
Solo self-employed	A dummy variable equal to 1 if the person currently works and reports being self-employed without employees.
Self-employed with employees	A dummy variable equal to 1 if the person currently works and reports being self-employed with employees.

5.3.2 Data Appendix 2: Technical Details on Analysis of Dependent Employment

In Section 4.4.4, we study the link between product market deregulation and dependent employment based on a broad set of individual-level and occupation-level outcomes. All individual-level results on dependent employment, as well as some of the occupation-level results, are based on survey data from the Microcensus SUF 2000–2008 provided by the Research Data Centers of the German Federal Statistical Office and the Statistical Offices of the German Federal States. These results are supplemented by occupation-level results obtained from the weakly anonymized version of the administrative data set SIAB provided by the Research Data Center of the German Federal Employment Agency at the Institute for Employment Research (IAB). Table A.22 describes all samples and respective sampling rules used in our analysis of dependent employment based on survey data (Microcensus). Table A.24 contains the respective information for the administrative data (SIAB). Table A.23 complements Table A.21 and defines further variables constructed for the analysis based on Microcensus data. Table A.25 contains the respective information on important SIAB-based variables.

At the individual level we investigate the link between the reform and the probability of being dependently employed, the probability of entry into dependent employment, the probability of exit out of dependent employment, as well as the probability of being newly dependently employed among all dependently employed craftsmen. Estimations for the probability of being dependently employed are based on the *base sample* which contains all craftsmen who are currently employed, unemployed, or out of the labor force. For our analysis of the probability of entry into self-employment we use the *entry sample*. The effect on the exit probabilities is estimated using the *dependent employment exit sample*, which only contains craftsmen who were self-employed 12 months ago. Lastly, the estimation sample for the probability of being newly dependently employed consists only of dependently employed craftsmen for whom information regarding the starting year of the current job is available.

To obtain differences-in-differences estimates at the occupational level, we aggregate the outcome variables accordingly. For the first-differences estimations, we then compute averages over the pre-reform and the post-reform period, respectively, to create the required two-period panel. Based on Microcensus data, the occupation-level outcomes are the number of dependently employed (head count) and the number of full-time employed (head count). Firstly, we identify full-time work based on the number of weekly working hours, and secondly, based on self-assessed part-time status. We also consider two kinds of full-time equivalents as described in more detail in Table A.23. Based on administrative data (SIAB), the occupation-level outcomes are the number of dependently employed (head count) and the number of full-time employed (head count). We also compute full-time equivalents that are based on full-time and part-time employed who are weighted according to the weighting scheme described in Table A.25.

Table A.22: Overview of All Samples Used in the Analysis of Dependent Employment Based on Survey Data (Microcensus)

Individual-level data:	
Base	**Scope:** all self-employed, dependently employed, unemployed, or out of the labor force during the period 2000–2008 with reported occupation belonging to control or treatment occupations **Sampling criteria:** male and female, aged 25–55, no institutionalized persons, no missing information in educational variable, no university or technical college degree; no civil servants, soldiers, apprentices, persons in military service or alternative civilian service, or persons helping in the family business **Number of observations:** about 195,000 observations in 23 control and 31 treatment occupations (about 43% of observations in treatment group)
Entry	**Scope:** self-employed, dependently employed, unemployed, or out of the labor force **Additional sampling criteria:** only individuals with non-missing information regarding their labor market status and (if applicable) type of employment 12 months ago*
New dependent employment	**Scope:** only currently dependently employed individuals **Additional sampling criteria:** only individuals with non-missing information regarding the year in which the current professional activity was started
Dependent employment (exit)	**Scope:** self-employed, dependently employed, unemployed, or out of the labor force, who report that they were dependently employed 12 months ago **Additional sampling criteria:** only individuals with non-missing information regarding their type of employment 12 months ago *
Occupation-level data:	
Occupation-year panel	Obtained by aggregating individual-level data by year and occupation

Note: * Questions regarding labor market activities 12 months before the interview were only posed to a 0.45% subsample of the population in the survey waves 2000–2004, and answering them was never mandatory.
"Additional sampling criteria" mentions all sampling criteria implemented on top of the base sampling criteria.

Table A.23: Definition of Further Important Variables Used in the Analysis of Dependent Employment Based on Survey Data (Microcensus)

Variable	Description
Individual-level variables:	
Dependent employment	A dummy variable equal to 1 if the person is currently working and reports being dependently employed.
Entry into dependent employment	A dummy variable equal to 1 if the person is currently working and reports being dependently employed, but was not dependently employed (either not working or self-employed) 12 months ago. The latter condition is constructed using information on the individual's labor market activities 12 months ago. In the survey waves 2000–2004, this information was only collected for a 0.45% subsample of the population, and answering the respective question wasn't mandatory over the whole period 2000–2008. With about 50% the share of missings is high, but they are proportionally distributed over treatment and control group.
Exit out of dependent employment	A dummy variable equal to 1 if the person is currently not dependently employed (either not working or self-employed), but was reported as working and being dependently employed 12 months ago. The latter condition is also constructed using information on the individual's labor market activities 12 months ago (see details above).
New dependent employment	A dummy variable equal to 1 if the person is currently working and reports being dependently employed, and the current job was started in the year in which the survey was taken. The latter condition is constructed using information on the starting year of the current job, which exhibits a share of missings of about 1.7%. Missings are proportionally distributed over treatment and control group.
Part time (self-assessed)	A dummy variable equal to 1 if the person reports that she works part time. The part-time status is based on the survey participants' self-assessment, i.e. we do not condition on the number of hours typically worked per week.
Part time ($\leq$ 30 hours)	A dummy variable equal to 1 if the person reports that she typically works 30 or less hours per week. This variable is based on information regarding the number of hours typically worked per week.
Occupation-level variables:	
Full-time equivalents	A weighted sum of all dependently employed individuals belonging to the respective occupation-year cell, where we use the individual weekly working hours divided by 40 as individual weights.
Full-time equivalents (SIAB)	In the SIAB data, we have no exact information about the hours worked. To create a version of the full-time equivalents variable that is comparable to the SIAB results, we therefore assign the following weights: full time ($>$ 39 hours) is weighted by 1, large part time ($>$ 17 and $\leq$ 39 hours) is weighted by $\frac{26}{39}$, and small part time ($\leq$ 17 hours) is weighted by $\frac{17}{39}$.

Table A.24: Overview of All Samples Used in the Analysis Based on Administrative Data (SIAB)

Individual-level data:	
SIAB base	**Scope:** dependently employed (full-time and part-time) working in an occupation belonging to control or treatment group during the period 2000–2008 **Sampling criteria:** male and female, aged 25–55; no missings in current occupation, type of employment, or highest educational degree; no apprentices, trainees, or home workers; no university or technical college degree **Number of observations:** about 300.000 person-year observations in 24 control and 35 treatment occupations (about 19.3% of person-year observations in treatment group)
Occupation-level data:	
SIAB occupation-year panel	Obtained by aggregating individual-level data by year and occupation

Table A.25: Definition of Important Variables Used in the Analysis of Dependent Employment Based on Administrative Data (SIAB)

Variable	Description
Individual-level variables:	
No vocational training	A dummy variable equal to 1 if the person does not hold a vocational training degree.
Vocational training	A dummy variable equal to 1 if the person does at least hold a vocational training degree (this is assumed to be true for all persons holding a master craftsman degree or working as a foreman).
Master craftsman/foreman	A dummy variable equal to 1 if the person is a master craftsman or works as a foreman.
Part time	A dummy variable equal to 1 if the person is classified as working either large (> 17 and ≤ 39 hours) or small part time (≤ 17 hours).
Occupation-level variables:	
Full-time equivalents	In the SIAB data we have no exact information about the hours worked. Employed individuals are classified as working full time (> 39 hours), large part time (> 17 and ≤ 39 hours) or small part time (≤ 17 hours). We therefore follow a weighting scheme according to which we assign the following weights: full time is weighted by 1, large part time is weighted by $\frac{26}{39}$, and small part time is weighted by $\frac{17}{39}$.

5.3.3 Data Appendix 3: Further Descriptive Evidence on Pre-reform Period

In this appendix, we provide further graphical evidence to check whether the outcomes of interest evolved similarly in the treatment and control occupations over the pre-reform period 2000–2003. Furthermore, we perform tests to check for statistically significant differences in pre-reform trends. That is, we run OLS estimations for the pre-reform period 2000–2003 according to the following model:

$$Y_{(i)ot} = \alpha + \beta_2 \cdot TG_o + \beta_5 \cdot trend_t + \beta_6 \cdot TG_o \cdot trend_t + u_{(i)ot},$$

where $Y_{(i)ot}$ is the outcome of interest and index i only applies if the estimation is run on individual-level data. The outcome variable can be either in levels, logs, or conventional growth rates. TG_o indicates the treatment occupations, $trend_t$ is a linear trend variable, and the coefficient β_6 on the interaction thereof is our coefficient of interest. If we run the checks on individual-level data, we cluster standard errors at the occupation level. If we run the tests on the occupations panel, we use robust standard errors.

Checks Regarding the Evolution of the Number of Registered Establishments

To investigate the comparability of treatment and control occupations during the pre-reform period 2000–2003, we run a series of regressions as described above to test for differences in pre-reform trends in the number of registered establishments, the number of apprenticeship graduates, the number of apprenticeship graduates per establishment, the number of master craftsman exams taken (including retakes), as well as the number of master craftsman exams per establishment. We run tests for the levels, the logs, and the yearly growth rates of these outcome variables, and we find no statistically significant differences in pre-reform trends.

Checks Regarding the Evolution of Self-employment

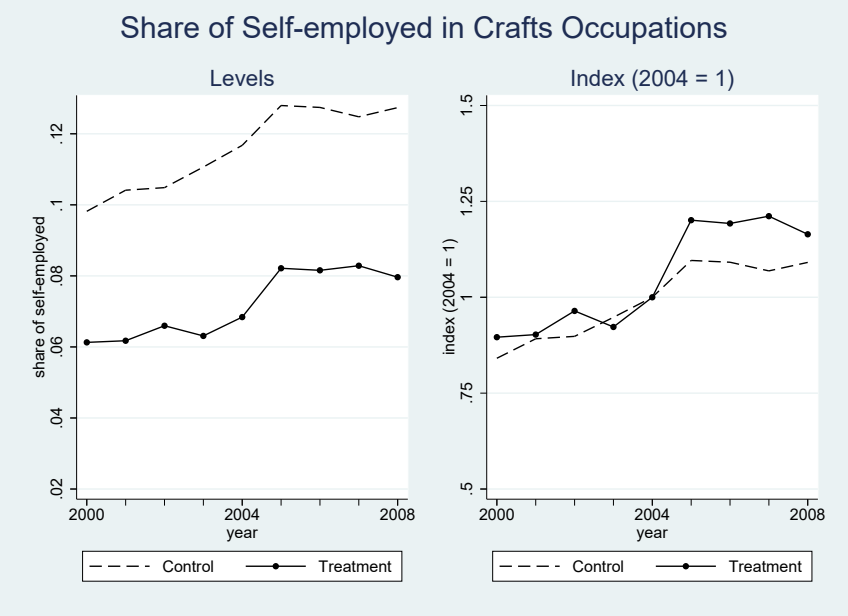
In addition to the graphical evidence provided in Section 4.3.1, here we present further graphs that show the evolution of the self-employment outcomes in more detail. Figures A.25–A.30 compare the respective outcomes between treatment and control group. According to Figure A.25, the share of self-employment in overall employment increased similarly in both groups of crafts occupations until 2004, and it increased relatively more strongly in the treatment occupations over the post-reform period. The probability of being self-employed, as shown in Figure A.26, grew similarly in both groups of crafts occupations over the whole period 2000–2008. In contrast to the previously described outcomes, the graphs on the probabilities of entry into self-employment, as shown in Figure A.27, and of exit out of self-

employment, as shown in Figure A.28, are less smooth, and the probabilities are especially volatile in the case of the treatment group. This makes it difficult to identify clear patterns from graphical inspection. The probability of exit out of self-employment seems to have decreased in both groups over the pre-reform period. In both cases, testing for differences in the linear time trends over the pre-reform period based on individual-level data reveals no statistically significant differences in pre-reform trends across the two groups of crafts occupations.

Figure A.29 shows the evolution of the average number of newly self-employed, and Figure A.30 shows the evolution of the share of new self-employment in overall self-employment. Apart from the spike in the number of newly self-employed in the control occupations in 2003, both graphs provide the picture that until 2004 the outcome evolved similarly in the treatment and the control occupations. It increased strongly in the treatment group during the post-reform period, and then dropped again in 2008.[2] However, we find a statistically significant difference in pre-reform trends: The share of newly self-employed in overall self-employment (in the *self-employment share sample*) increased significantly less in the treatment than in the control occupations over the pre-reform period. We obtain a linear trend estimate of 0.005**, and an estimated coefficient of -0.008** on the interaction term between the linear trend variable and the treatment group dummy. If we exclude the year 2000 from the estimation sample, the difference in trends turns statistically insignificant.

[2] As Figure A.31 shows, the 2003 spike does not vanish even if we exclude from the sample all self-employed craftsmen who presumably received the start-up subsidy Me Inc., which was introduced in 2003.

Figure A.25: Share of Self-employed in Overall Employment in Crafts Occupations over the Period 2000–2008

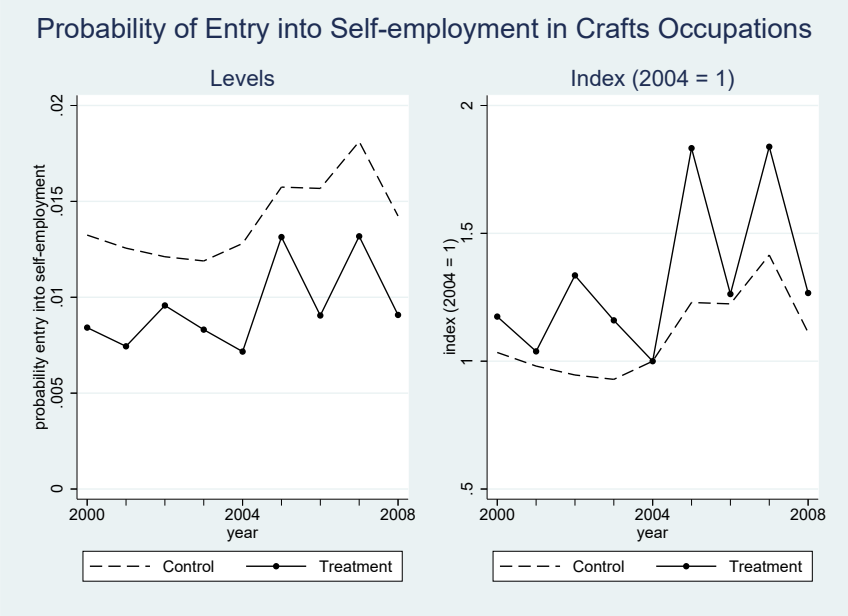


Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average share of self-employed craftsmen in overall employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.26: Probability of Self-employment in Crafts Occupations over the Period 2000–2008

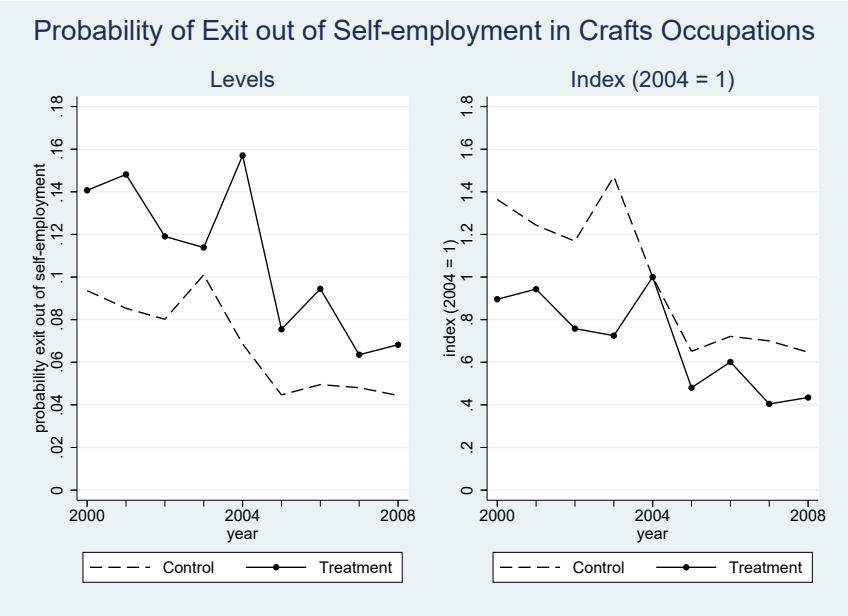
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average probability of self-employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.27: Probability of Entry into Self-employment in Crafts Occupations over the Period 2000–2008



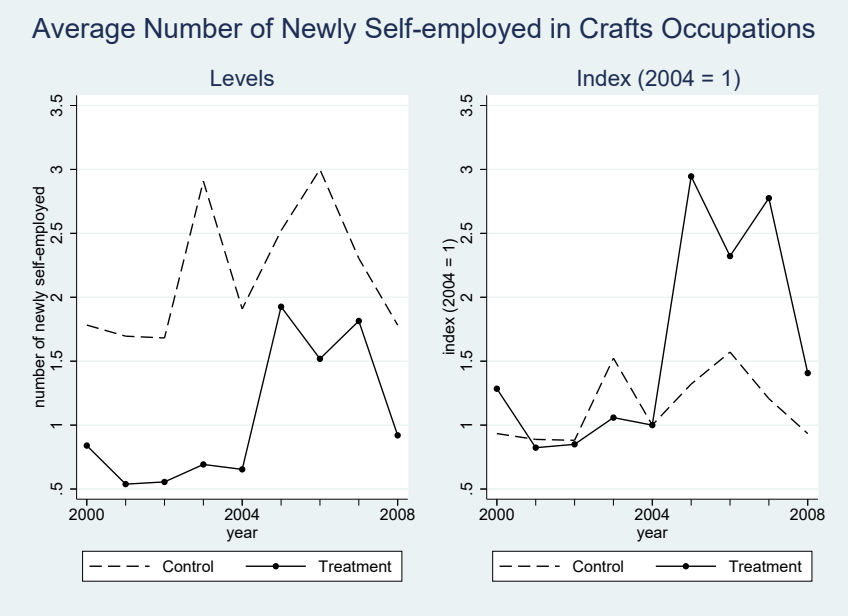
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average probability of entry into self-employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.28: Probability of Exit out of Self-employment in Crafts Occupations over the Period 2000–2008



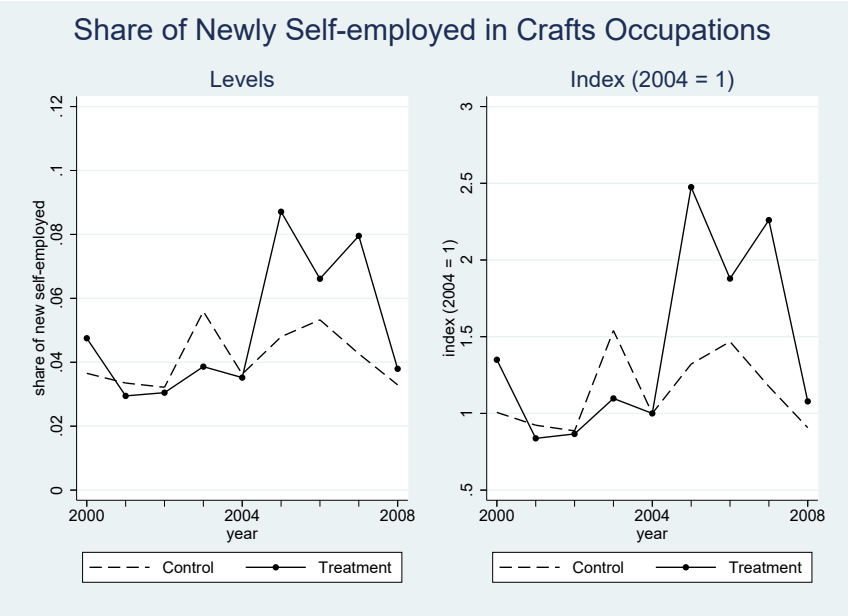
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average probability of exit out of self-employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.29: Average Number of Newly Self-employed in Crafts Occupations over the Period 2000–2008



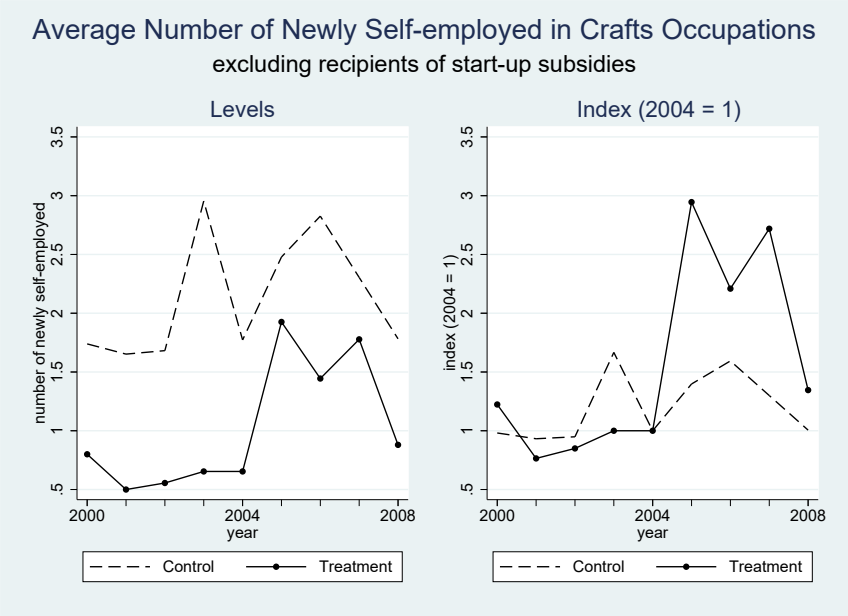
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average number of newly self-employed per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.30: Share of New Self-employment in Overall Self-employment in Crafts Occupations over the Period 2000–2008



Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average share of new self-employment in overall self-employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.31: Average Number of Newly Self-employed in Crafts Occupations over the Period 2000–2008 Excluding Presumed Recipients of Start-up Subsidiy Me Inc.



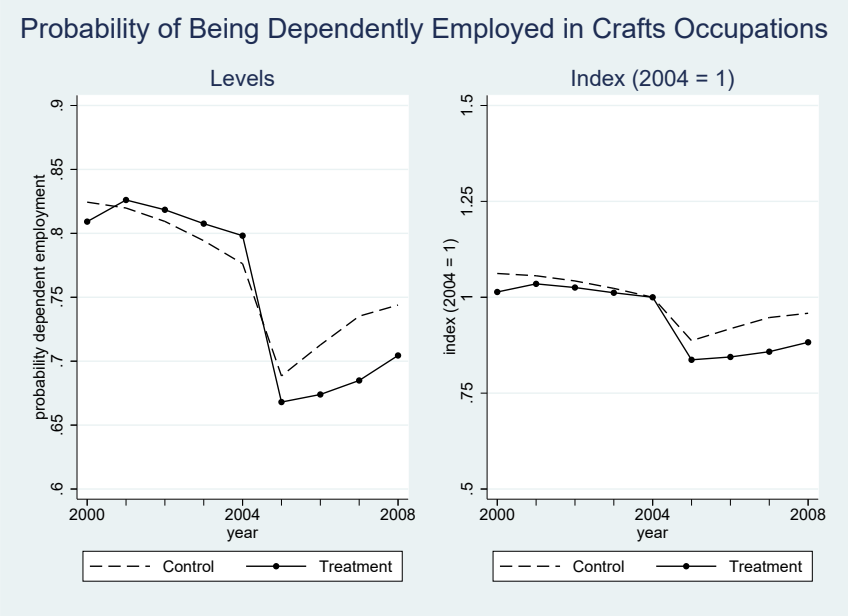
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average number of newly self-employed in overall self-employment per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel). Presumed recipients of start-up subsidy Me Inc. have been excluded from the data.

Finally, based on the occupations panel we also run trend regressions for the number of self-employed to check whether there is a statistically significant difference in trends between treatment and control occupations over the pre-reform period. We only consider the outcome in levels, since taking logs or computing growth rates is not feasible due to a few occupation-year-cells with zero self-employed. Out of 450 occupation-year-cells over the period 2000–2008, 10 cells contain no self-employed at all. We do not find a statistically significant difference in the linear pre-reform trends.

Checks Regarding the Evolution of Dependent Employment

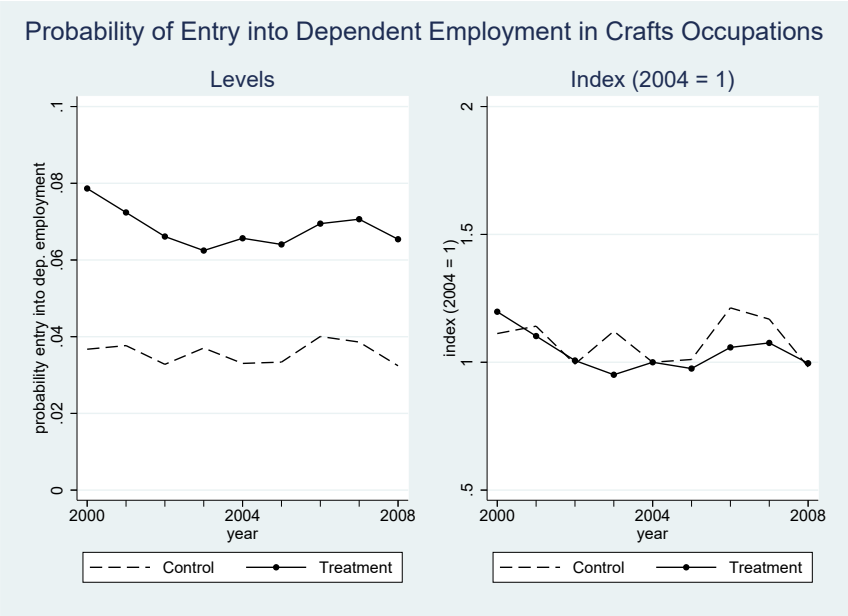
In addition to the graphical evidence provided in Section 4.3.1, here we provide further graphs that show the evolution of the dependent employment outcomes. Figures A.32–A.35 compare the respective dependent employment outcomes between treatment and control group. According to Figure A.32, the probability of being dependently employed evolved similarly in both groups of crafts occupations until 2004. However, when we test for differences in pre-reform trends, we find a small but statistically significant difference in trends: Over the pre-reform period, the probability of being dependently employed decreased significantly less in the treatment occupations relative to the control occupations. We obtain a linear trend estimate of -0.01***, and an estimated coefficient of 0.009*** on the interaction term between the linear trend variable and the treatment group dummy. However, the difference in trends turns statistically insignificant if we exclude the year 2000 from the estimations. The probability of entry into dependent employment, as shown in Figure A.33, is more volatile. It is not clear whether the two groups really followed similar pre-reform trends. When we test for differences in pre-reform trends, we find that the probability of entry into dependent employment decreased significantly more strongly in the treatment than in the control occupations over the pre-reform period. We obtain a linear trend estimate of -0.0004, and an estimated coefficient of -0.005** on the interaction term between the linear trend variable and the treatment group dummy. In this case, the difference in trends does not lose statistical significance if we exclude the year 2000 from the estimations. In contrast to this, as Figure A.34 shows, the probability of exit out of dependent employment evolved similarly in both groups over the pre-reform period. Figure A.35 shows the evolution of the share of new dependent employment in overall dependent employment. Again, we observe a small spike in the share of newly dependently employed in the control occupations in 2003. Apart from this, the graph indicates that until 2004 the outcome evolved similarly in the treatment and the control occupations.

Figure A.32: Probability of Being Dependently Employed in Crafts Occupations over the Period 2000–2008 Based on Survey Data



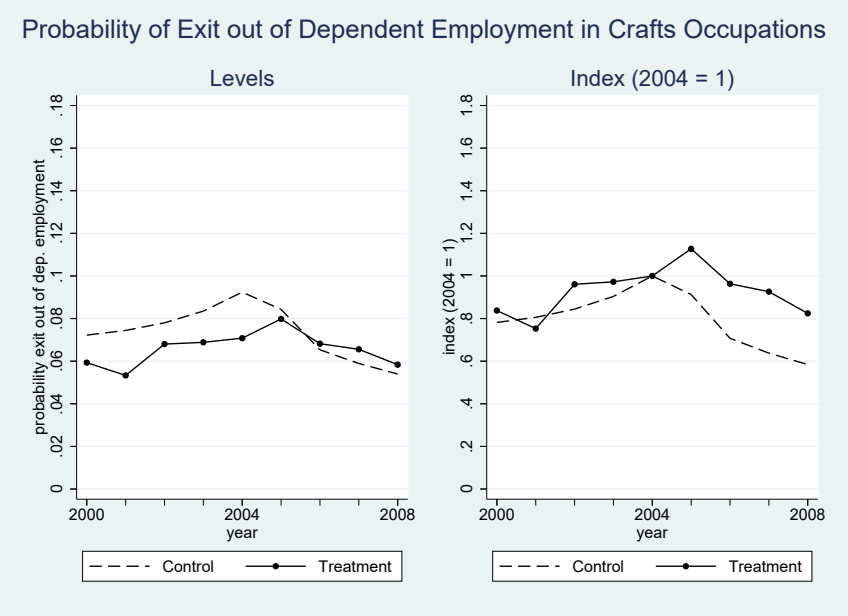
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average probability of being dependently employed in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.33: Probability of Entry into Dependent Employment in Crafts Occupations over the Period 2000–2008 Based on Survey Data



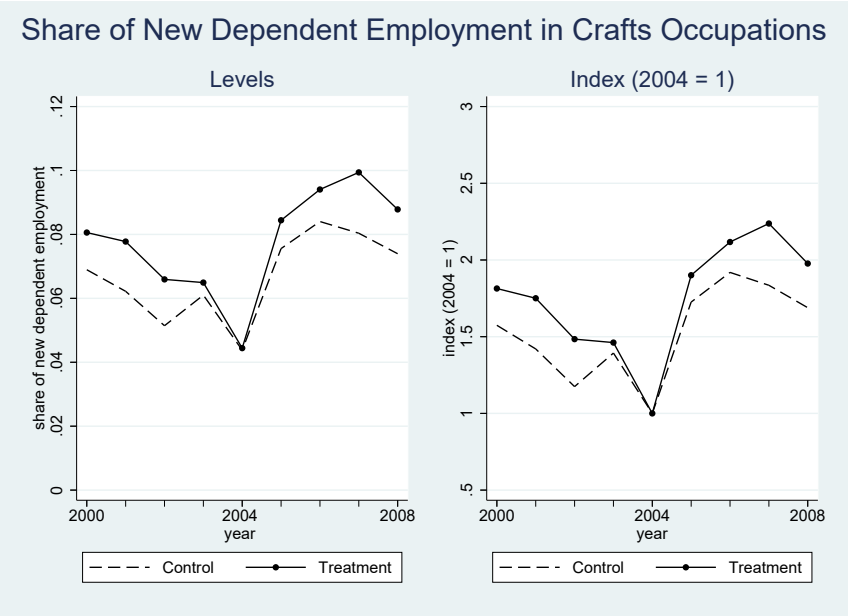
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average probability of entry into dependent employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.34: Probability of Exit out of Dependent Employment in Crafts Occupations over the Period 2000–2008 Based on Survey Data



Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average probability of exit out of dependent employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.35: Share of New Dependent Employment in Overall Dependent Employment in Crafts Occupations over the Period 2000–2008 Based on Survey Data



Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average share of new dependent employment in overall dependent employment in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Overall, the trend estimations based on individual-level survey data from the Microcensus indicate that the common trends assumption is only plausible with respect to some of the employment-related outcomes. In addition to this, we also run trend regressions on the occupations panel. We consider a wide range of dependent employment outcomes in levels, logs, and conventional growth rates: the total number of self-employed, the total number of hours worked, full-time equivalents, full-time equivalents that are comparable to the SIAB-results, and the total number of full-time employed. Due to a number of empty occupation-year-cells in the case of the part-time employment variables, we can only perform the trend estimations for the part-time employment variables in levels. For all of the aforementioned outcome variables, we do not find statistically significant differences between treatment and control occupations in the linear pre-reform trends. We also perform similar tests on the occupations panel obtained from administrative data (SIAB). We consider several dependent employment outcomes in levels, logs, and conventional growth rates: the total number of self-employed, full-time equivalents, and the total number of full-time employed. Again, due to a number of empty occupation-year cells in the case of the part-time employment variable, we can only study the pre-reform trends in this outcome variable in levels. As was the case with the Microcensus survey data, for all of the aforementioned outcome variables we do not find statistically significant differences in the linear pre-reform trends between treatment and control occupations.

5.3.4 Data Appendix 4: Placebo Tests

As a further check on the plausibility of the common trends assumption, we estimate the effects of placebo treatments which correspond to the main results provided in the various regression tables in Section 4.4. For all differences-in-differences models, we analyze two placebo scenarios:

i. We recode the post-reform dummy (PR) and treat the data as if the reform had already come into force on 1. January 2002. The estimation sample then includes the years 2000–2001 as the pre-reform years, and the years 2002–2003 as the post-reform years.

ii. In a second version of this test, we treat the data as if the reform had already come into force on 1. January 2003. The estimation sample then includes the years 2000–2002 as the pre-reform years, and the years 2003–2004 as the post-reform years.

For the first-differences estimations, which are based on a two-period panel, we implement the following placebo test:

iii. We treat the data as if the reform had come into force on 1. January 2002. The two-period panel is constructed accordingly: The pre-reform period averages the years 2000 and 2001, whereas the post-reform period averages the years 20002 and 2003.

If the treatment and control occupations evolved similarly over the pre-reform period, the placebo regressions should yield statistically insignificant effects of interest with point estimates that are close to zero. This should hold especially for the versions (i) and (iii) of the placebo tests, since they do not include any of the actual post-reform years 2004–2008 in the estimation samples.

In this appendix, five tables provide an overview of the placebo estimation results. Table A.26 provides placebo test results for the regressions on growth in the number of registered establishments (compare to Table 4.4 in Section 4.4.2). Panel A shows that in the case of the differences-in-differences estimations, the placebo reform in 2002 yields the expected insignificant results. The placebo reform in 2003 yields significant positive results. However, this test still includes the year 2004, which is part of the post-reform period. The results of the 2002 test are of higher relevance. The first-differences placebo test shown in Panel B yields small but statistically significant negative results, while the first-differences estimation shown in Table 4.4 yields a large positive reform effect. The latter result suggests that the two groups of crafts occupations did not evolve fully similarly over the pre-reform period with respect to relative changes in the number of registered establishments – although the placebo first-differences estimate is of comparatively small magnitude relative to the true first-differences estimate.

Table A.27 provides placebo regression results on self-employment based on occupation-level data (compare to Tables 4.5 and 4.6). The placebo differences-in-differences regressions in Panel A lead to small and statistically insignificant estimates. However, the placebo test for the first-differences estimations displayed in Panel B is problematic. While the effect of

interest is statistically insignificant, the point estimate is quite large. This suggests that with respect to relative changes in the total number of self-employed, the two groups of crafts occupations evolved differently over the pre-reform period.

Table A.28 provides placebo regression results on dependent employment outcomes based on occupation-level data from the survey data set Microcensus (compare to Tables 4.9 and 4.10). Panel A shows that in the case of the differences-in-differences estimations, the placebo reform in 2002 yields statistically insignificant negative results that are almost of the same magnitude as the real reform effects displayed in Table 4.9. The other placebo results in Panel A and Panel B are less problematic. In addition to this, Table A.29 provides placebo regression results on dependent employment outcomes based on occupation-level data from the administrative data set SIAB (compare to Table 4.11). These tests reveal the same problematic pattern: The differences-in-differences estimations for the placebo reform in 2002 yield statistically significant negative results that are almost of the same magnitude as the real reform effects displayed in Table 4.11. Again, the other placebo results in Panel A and Panel B are less problematic, but all in all the placebo tests indicate that there was a lot of pre-reform heterogeneity in the evolution of dependent employment outcomes between he two groups of crafts occupations.

Table A.30 summarizes all placebo test results regarding the individual-level regressions. All individual-level results on self-employment and dependent employment are based on Microcensus survey data (compare to Tables 4.12 and 4.12). Panel A shows the test results on self-employment. Here, the tests yield small and statistically insignificant estimates of interest. Panel B shows the test results on dependent employment. Both placebo tests for the probability of entry into dependent employment yield statistically significant estimates that are even more pronounced than the estimates in Table 4.12. Similar results hold for the placebo test of type (ii) on the share of new employment. This indicates that the respective outcomes evolved significantly differently between treatment and control occupations over the pre-reform period. Again, this reveals considerable pre-reform heterogeneity in the evolution of dependent employment outcomes between the two groups of occupations.

To sum up our findings, the placebo tests indicate that there was considerable pre-reform heterogeneity in the evolution of labor market outcomes between treatment and control occupations. This holds especially for the dependent employment outcomes, but the first-differences placebo estimation for self-employment also yields problematic results.

Table A.26: Occupation-level Placebo Results on Growth in the Number of Registered Establishments in Crafts Occupations

	(1)	(2)
A. Differences-in-differences regressions:		
Placebo reform in 2002	-0.005	-0.006
	(0.005)	(0.005)
N	246	222
Adj. R^2	0.603	0.563
Placebo reform in 2003	0.067***	0.041***
	(0.018)	(0.012)
N	328	296
Adj. R^2	0.293	0.322
Year fixed effects	yes	yes
Occupation fixed effects	yes	yes
Check: EU expansion	no	yes
Dependent variable in	– growth rates –	
B. First-differences regressions:		
Placebo reform in 2002	-0.031***	-0.036***
	(0.012)	(0.012)
N	82	74
Adj. R^2	0.064	0.098
Check: EU expansion	no	yes
Dependent variable in	– growth rates –	

Data source: ZDH, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimation results from OLS estimation with constant. Panel A: DiD estimations based on occupations panel, dependent variable: yearly growth rate of number of establishments, robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the estimation sample includes only years 2000–2003, with years 2002–2003 being the post-reform period. "Placebo reform 2003" treats the data as if the reform had come into force on 1.1.2003 – the estimation sample includes only years 2000–2004, with years 2003–2004 being the post-reform period. Wave and occupation fixed effects included. Panel B: FD estimation based on two-period panel, dependent variable: growth rate of number of establishments (comparing pre-and post-reform period), robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the two-period panel aggregates years 2000–2001 versus years 20002–2003. Check: EU expansion "yes" indicates that we exclude treatment occupations with a high share of newly registered establishments coming from new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia.

Table A.27: Occupation-level Placebo Results on the Number of Self-employed in Crafts Occupations

	(1)	(2)
A. Differences-in-differences regressions:		
Placebo reform in 2002	0.703	0.812
	(1.500)	(1.507)
N	200	200
Adj. R2	0.987	0.987
Placebo reform in 2003	-0.159	0.002
	(1.463)	(1.471)
N	250	250
Adj. R2	0.986	0.986
Year fixed effects	yes	yes
Occupation fixed effects	yes	yes
Check: EU expansion	no	yes
Check: Subsidies	no	yes
Dependent variable in	– levels –	
B. First-differences regressions:		
Placebo reform in 2002	0.319	0.320
	(0.223)	(0.223)
N	50	50
Adj. R2	0.015	0.016
Check: EU expansion	no	yes
Check: Subsidies	no	yes
Dependent variable in	– growth rates –	

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimation results from OLS estimation with constant. Panel A: DiD estimations based on occupations panel as described in Table A.20, dependent variable: number of self-employed, robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the estimation sample includes only years 2000–2003, with years 2002–2003 being the post-reform period. "Placebo reform 2003" treats the data as if the reform had come into force on 1.1.2003 – the estimation sample includes only years 2000–2004, with years 2003–2004 being the post-reform period. Wave and occupation fixed effects included. Panel B: FD estimation based on two-period panel, dependent variable: growth rate of number of self-employed, robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the two-period panel aggregates years 2000–2001 versus years 20002–2003. Check: EU expansion "yes" indicates that we exclude treatment occupations with a high share of newly registered establishments coming from new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. Check: Subsidies "yes" indicates that we exclude individuals who presumably receive Me Inc. start-up subsidies.

Table A.28: Occupation-level Placebo Results on Growth in Dependent Employment Outcomes in Crafts Occupations Based on Survey Data

	(1) Total	(2) FTE	(3) FTE (SIAB)	(4) Full-time (> 30 h)	(5) Full-time (self-rep.)
A. Differences-in-differences regressions:					
Placebo reform in 2002	-0.074	-0.081	-0.075	-0.080	-0.083
	(0.051)	(0.052)	(0.051)	(0.053)	(0.052)
N	153	153	153	153	0.036
Adj. R^2	-0.080	-0.088	-0.084	-0.123	(0.039)
Placebo reform in 2003	0.019	0.018	0.030	0.020	0.018
	(0.046)	(0.045)	(0.044)	(0.045)	(0.045)
N	204	204	204	204	204
Adj. R^2	-0.179	-0.167	-0.168	-0.171	-0.165
Year fixed effects	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes
Dependent variable in	– growth rates –				
B. First-differences regressions:					
Placebo reform in 2002	-0.007	-0.009	-0.014	-0.011	-0.013
	(0.040)	(0.040)	(0.039)	(0.040)	(0.040)
N	51	51	51	51	51
Adj. R^2	-0.020	-0.019	-0.018	-0.019	-0.018
Dependent variable in	– growth rates –				

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimation results from OLS estimation with constant. Panel A: DiD estimations based on occupations panel as described in Table A.22, dependent variables: yearly growth rate of respective variable (see column title), robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the estimation sample includes only years 2000–2003, with years 2002–2003 being the post-reform period. "Placebo reform 2003" treats the data as if the reform had come into force on 1.1.2003 – the estimation sample includes only years 2000–2004, with years 2003–2004 being the post-reform period. Panel B: FD estimation based on two-period panel, dependent variables: growth rate of respective variable (comparing pre-and post-reform period), robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the two-period panel aggregates years 2000–2001 versus years 20002–2003.

Table A.29: Occupation-level Placebo Results on Growth in Dependent Employment Outcomes in Crafts Occupations Based on Administrative Data

	(1) Total	(2) FTE	(3) Full-time
A. Differences-in-differences regressions:			
Placebo reform in 2002	-0.028	-0.031	-0.038
	(0.024)	(0.025)	(0.026)
N	177	177	177
Adj. R2	-0.039	-0.021	-0.008
Placebo reform in 2003	-0.010	-0.014	-0.019
	(0.019)	(0.019)	(0.020)
N	236	236	236
Adj. R2	-0.038	-0.046	-0.063
Year fixed effects	yes	yes	yes
Occupation fixed effects	yes	yes	yes
Dependent variable in	– growth rates –		
B. First-differences regressions:			
Placebo reform in 2002	0.005	0.007	0.009
	(0.024)	(0.024)	(0.025)
N	59	59	59
Adj. R2	-0.017	-0.016	-0.015
Dependent variable in	– growth rates –		

Data source: SIAB 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All estimation results from OLS estimation with constant. Panel A: DiD estimations based on occupations panel as described in Table A.24, dependent variables: yearly growth rate of respective variable (see column title), robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the estimation sample includes only years 2000–2003, with years 2002–2003 being the post-reform period. "Placebo reform 2003" treats the data as if the reform had come into force on 1.1.2003 – the estimation sample includes only years 2000–2004, with years 2003–2004 being the post-reform period. Panel B: FD estimation based on two-period panel, dependent variables: growth rate of respective variable (comparing pre-and post-reform period), robust standard errors. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the two-period panel aggregates years 2000–2001 versus years 20002–2003.

Table A.30: Individual-level Placebo Differences-in-differences Results Based on Survey Data

	Differences-in-differences estimation				
	(1)	(2)	(3)	(4)	(5)
	Share	Probability	Entry	Exit	New (self-)employment
A. Self-employment:					
Placebo reform in 2002	0.002	0.003	0.003	-0.027	-0.012
	(0.005)	(0.005)	(0.002)	(0.022)	(0.008)
N	74,943	84,047	42,440	2,708	6,456
Adj. R^2	0.016	0.016	0.001	0.043	0.018
Placebo reform in 2003	-0.004	-0.002	-0.000	-0.014	-0.012
	(0.004)	(0.003)	(0.002)	(0.024)	(0.009)
N	92,142	103,827	52,700	3,409	8,096
Adj. R^2	0.018	0.017	0.001	0.041	0.020
Year fixed effects	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes
Other controls	yes	yes	yes	yes	yes
Check: EU expansion	no	no	no	no	no
Check: Subsidies	no	no	no	no	no
Dependent variable in	– levels –				
B. Dependent employment:					
Placebo reform in 2002		0.009	-0.012**	0.005	-0.007
		(0.006)	(0.005)	(0.005)	(0.006)
N		84,047	42,440	30,608	66,188
Adj. R^2		0.008	0.005	0.002	0.008
Placebo reform in 2003		0.013	-0.010**	-0.004	-0.013**
		(0.008)	(0.004)	(0.005)	(0.005)
N		103,827	52,700	37,686	81,235
Adj. R^2		0.009	0.005	0.002	0.009
Year fixed effects	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes
Other controls	yes	yes	yes	yes	yes
Dependent variable in	– levels –				

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on individual-level data, dependent dummy variables: see column title and panel title, as well as descriptions in Tables A.21 and A.23, standard errors clustered at occupation level. Other controls include age, age^2, dummy variables for foreign citizenship, no vocational training. "Placebo reform 2002" treats the data as if the reform had come into force on 1.1.2002 – the estimation sample includes only years 2000–2003, with years 2002–2003 being the post-reform period. "Placebo reform 2003" treats the data as if the reform had come into force on 1.1.2003 – the estimation sample includes only years 2000–2004, with years 2003–2004 being the post-reform period.

5.3.5 Data Appendix 5: Changes in the Sample Composition Over the Pre-reform Period

Tables 4.2, 4.3, and A.32 show that treatment and control occupations differ with respect to the levels of many sample characteristics. In this appendix, we check whether the demographic composition of the treatment and the control group evolved systematically differently over the pre-reform period as well. For example, we check for differences in the probability of being a female craftsman based on individual-level data, and for differences in the share of female craftsmen at the occupational level. Firstly, based on individual-level data we run OLS estimations on a limited sample of only two subsequent waves $t-1$ and t according to the following model:

$$Y_{iot} = \alpha + \beta_2 \cdot TG_o + \beta_7 \cdot wave\_t_t + \beta_8 \cdot TG_o \cdot wave\_t_t + u_{(i)ot},$$

that is, we implement pairwise comparisons of subsequent waves for the waves 2000–2003. $wave\_t_t$ is a dummy variable indicating wave t. β_8 is the coefficient of interest. Since the dependent variable Y_{iot} is in levels, we analyze absolute changes in the respective outcome variable. When we run test regressions on individual-level data, we use standard errors clustered at the occupational level. In the case of the ZDH data, which is only available at the occupational level, we run regressions on an occupations panel using robust standard errors. Additionally, we also analyze relative changes by using growth rates as the dependent variable. We aggregate individual-level data at the occupation-year level and compute year-to-year growth rates for the outcomes of interest. The model then simplifies to:

$$\left(\frac{Y_{ot} - Y_{ot-1}}{Y_{ot-1}} \right) = \alpha + \beta_2 \cdot TG_o + u_{ot},$$

where, again, we implement pairwise comparisons of subsequent waves for the waves 2000–2003 with robust standard errors. This allows us to implement simple tests for differential year-to-year changes in the compositional characteristics of the two groups of crafts occupations. Below, we summarize the main test results.

ZDH data

Based on data provided by the German Confederation of Skilled Crafts, we first test for differential year-to-year changes in the number of apprenticeship graduates, the number of apprenticeship graduates per establishment, the number of master craftsman exams taken (including retakes), as well as the number of master craftsman exams per establishment. We find no systematic statistically significant differences in absolute and relative yearly changes of the above-mentioned characteristics during the pre-reform period.

Survey data (Microcensus)

We next consider the samples presented in Table 4.2, Panels A and B, and Table 4.3, Panel C, which are all based on survey data (Microcensus). While we observe differences in the levels of characteristics across the two groups of crafts occupations, we do not find systematic differences between treatment and control occupations in the absolute and relative year-to-year changes of a wide range of compositional characteristics. These characteristics include the shares of self-employed, dependently employed, and unemployed craftsmen, as well as the share of craftsmen currently out of the labor force, the share of female craftsmen, the average age, the shares of five-year age groups, the share of low-skilled craftsmen, the shares of federal states, and the share of part-time working craftsmen. These test results hold for both the base sample as well as for the sub-samples – the exception being that in the base sample (Panel A) the share of part-time working craftsmen increased significantly more strongly in the treatment occupations than in the control occupations in the years 2000–2001 and 2001–2002. Also, both in the base sample (Panel A) and in the sub-sample of dependently employed (Panel C) the share of craftsmen aged 25–30 years increased significantly more strongly in the treatment occupations relative to the control occupations in the years 2001–2002 and 2002–2003. Furthermore, in the dependent employment sample (Panel C) the share of observations from the federal state of "Saxony" changed significantly differently in the treatment occupations relative to the control occupations in the years 2000–2001 and 2002–2003.

Administrative data (SIAB)

We also run a series of tests to check whether the composition of the two groups of crafts occupations did evolve systematically differently over the pre-reform period in the dependent employment sample based on administrative data. We again consider the share of female craftsmen, the average age, the shares of five-year age groups, the share of craftsmen with foreign citizenship, the share of low-skilled craftsmen, the shares of federal states, and the share of part-time working craftsmen. While we observe differences in the levels of characteristics across the two groups of crafts occupations, we find almost no systematic differences between treatment and control occupations in the absolute and relative year-to-year changes of these sample characteristics – the exception being that in the sample presented in Table A.32, Panel D the share of observations from the federal state of "Lower Saxony" changed significantly differently in the treatment occupations relative to the control occupations in the years 2001–2002 and 2002–2003. The share of observations from the federal state of "Bavaria" changed significantly differently in the years 2000–2001 and 2002–2003.

5.3.6 Table Appendix

Table A.31: Overview of Treatment, Control, and Reassigned Crafts Occupations

53 treatment occupations:
Accordion and concertina makers, Basket weavers, Bookbinders, Bow makers, Brewers and maltsters, Cast stone and terrazzo makers, Cleaners, Clockmakers, Container and apparatus makers, Coopers, Custom tailors, Cutting tool mechanics, Decorative metalworkers, Electroplaters, Embroiderers, Engravers, Flexographers, Founders and bell founders, Furrier, Gilders, Glass and porcelain painters, Glass finishers, Gold- and silversmiths, Hatters, Interior Decorators, Metal wind instrument makers, Millers, Model makers, Optics technicians/precision opticians, Organ and harmonium makers, Parquet recliners, Photographers, Piano and harpsichord makers, Plucked instrument makers, Potters, Precious stone cutters and engravers, Printers, Printers, roller shutters and blinds fitters, Saddlers and bag makers, Sail makers, Screed Layers, Shoemakers, Sign and neon sign makers, Textile cleaners, Tile, slab and mosaic layers, Turners, ivory sculptors and wooden toy makers, Violin makers, Wax chandlers, Weavers, Wine cellarmen, Wood sculptors, Wooden wind instrument makers
29 control occupations:
Agricultural machinery mechanics, Automotive technicians, Boat- and shipbuilders, Bricklayers and concretors, Carpenters, Chimney sweepers, Coachbuilders, Cooling systems fitters, Dental technicians, Electrical machine building, Electricians, Glass blowers and glass apparatus makers, Glaziers, Hearing aid acousticians, Installers and heating fitters, Joiners/cabinet makers, Mechanics for tires and vulcanization, Metalworkers, Motorcycle and bicycle mechanics, Opticians, Orthopedic shoemakers, Orthopedic technicians, Plumbers, Precision mechanics, Road builders, Roofers, Ropemakers, Scaffolders, Stove heating and hot air heating fitters
12 reassigned occupations:
Bakers, Butchers, Gunsmiths, Hairdressers, Information technicians, Painters and lacquerers, Pastry bakers, Plasterers, Stonemasons and stone carvers, Surgical instrument makers, Thermal and acoustic insulation fitters, Well sinkers

Note: Due to data limitations not all of the above-listed treatment and control occupations enter the estimation samples. Reassigned occupations are fully excluded from the analysis.

Table A.32: Descriptives Comparing Pre- and Post-reform Period Based on Administrative Data

	Treatment		Control	
	Pre	Post	Pre	Post
D. Dependently employed craftsmen only				
Age (mean)	40.2	41.3	38.9	40.1
Female	34.2	31.9	4.2	4.6
Foreign	12.0	11.1	5.9	5.2
No vocational training	19.3	16.5	3.9	3.3
Vocational training degree	80.7	83.5	96.1	96.7
Master craftsman/foreman	2.1	2.3	5.4	5.4
Part-time	10.6	11.4	1.3	1.7
N (total)	29,288	24,118	121,083	101,648
Sample share	19.5	19.2	80.5	80.8

Data source: SIAB 2000–2008. Reported numbers are given in % if not noted otherwise. Pre-reform period refers to the years 2000–2003, post-reform period refers to the years 2005–2008.

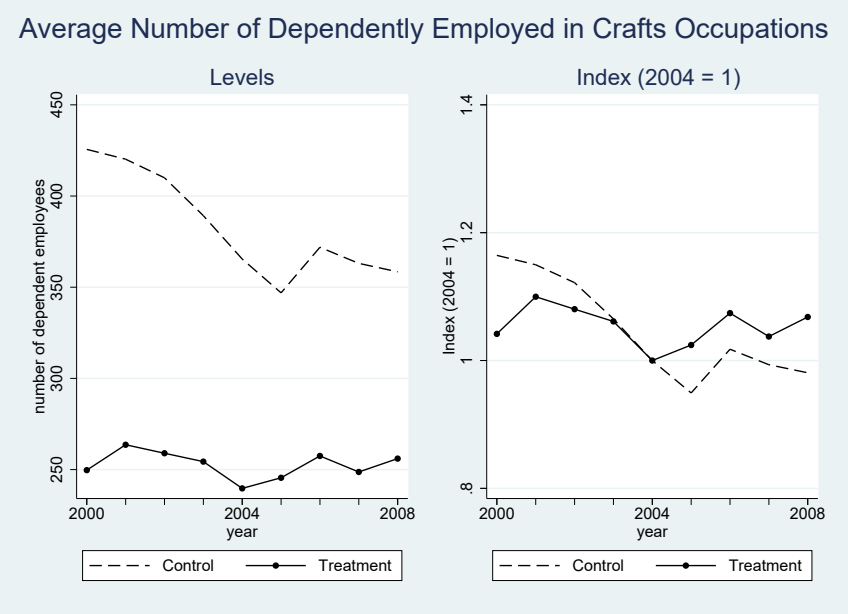
Table A.33: Selected Individual-level DiD(iD) Results on Self-employment in Crafts Occupations

	Differences-in-differences(-in-differences) estimation				
	(1)	(2)	(3)	(4)	(5)
	Probability	Probability	Probability	Entry	Entry
PR * TG	-0.001	0.001	-0.000	-0.001	0.001
	(0.004)	(0.008)	(0.005)	(0.002)	(0.002)
Female	-0.077***	-0.079***	-0.032***	-0.009***	-0.004**
	(0.012)	(0.019)	(0.006)	(0.002)	(0.002)
Female * TG * PR		0.017*			
		(0.010)			
Female * TG		0.013			
		(0.024)			
Female * PR		-0.023***			
		(0.006)			
With empl.			0.944***		0.107***
			(0.010)		(0.008)
With empl. * TG * PR			0.015*		-0.023*
			(0.008)		(0.014)
With empl. * TG			-0.081**		0.002
			(0.031)		(0.018)
With empl. * PR			-0.007***		0.005
			(0.002)		(0.008)
Year fixed effects	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes
Wave 2004 included	no	no	no	no	no
Other controls	yes	yes	yes	yes	yes
Check: EU expansion	yes	yes	yes	yes	yes
Check: Subsidies	yes	yes	yes	yes	yes
N	173,294	173,294	173,294	128,120	128,120
Adj R2	0.017	0.017	0.505	0.001	0.036

Data source: Microcensus SUF 2000–2008, own calculations. Statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. All DiD estimation results from OLS estimation with constant based on individual-level data, different estimation samples described in Table A.20, dependent dummy variables: see column title and description in Table A.21, wave 2004 excluded, standard errors clustered at occupation level. Occupation fixed effects "yes" indicates that the model has been within-transformed with respect to occupation-specific means. Check: EU expansion "yes" indicates that we exclude individuals coming from four new Eastern EU member states: Czech Republic, Hungary, Poland, and Slovakia. Check: Subsidies "yes" indicates that we exclude individuals who presumably receive Me Inc. start-up subsidies.

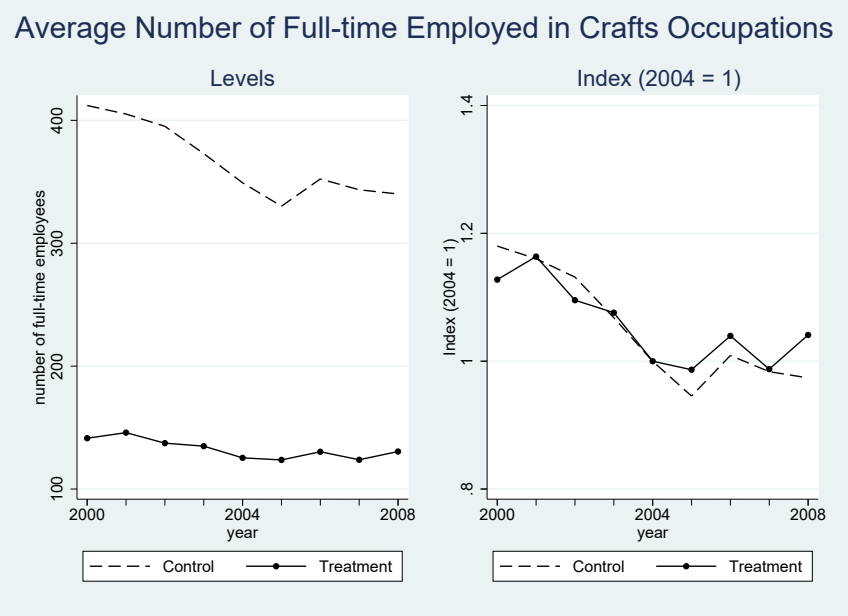
5.3.7 Figure Appendix

Figure A.36: Average Number of Dependently Employed in Crafts Occupations over the Period 2000–2008 Based on Survey Data



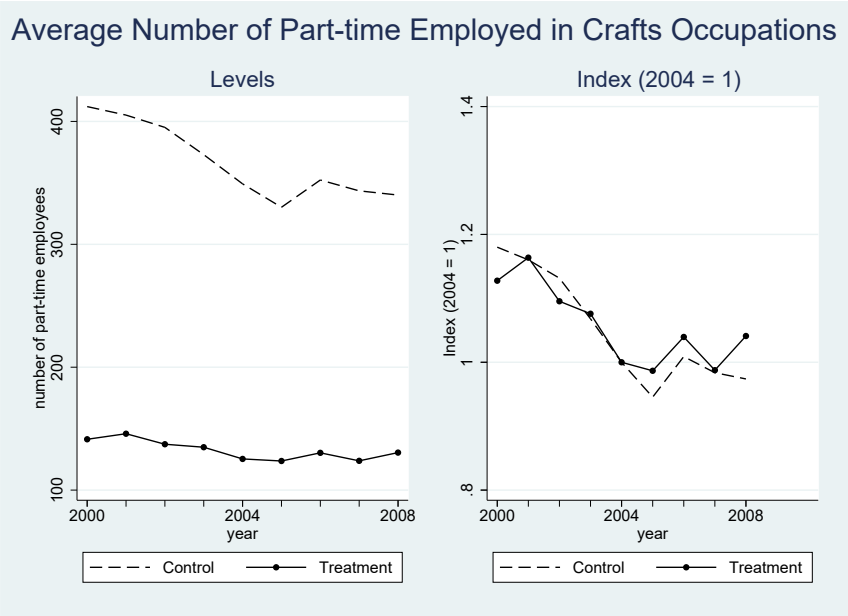
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average number of dependently employed (full-time and part-time) per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.37: Average Number of Full-time Employed in Crafts Occupations over the Period 2000–2008 Based on Survey Data



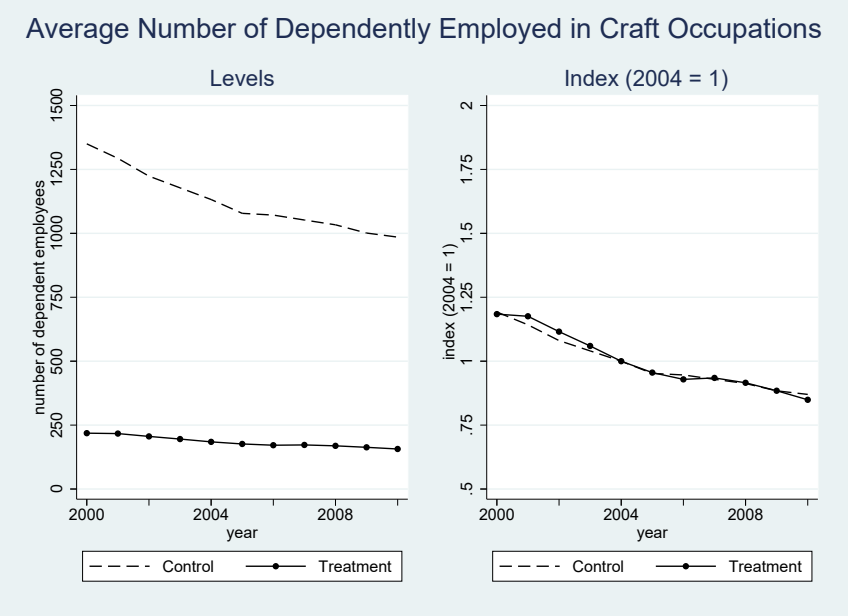
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average number of full-time employed per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.38: Average Number of Part-time Employed in Crafts Occupations over the Period 2000–2008 Based on Survey Data



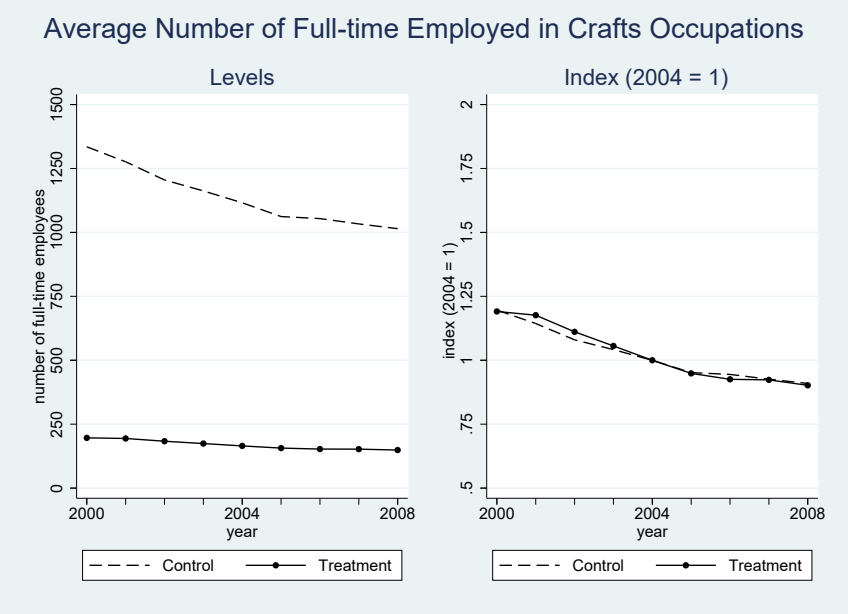
Data source: Microcensus SUF 2000–2008, own calculations. This graph shows the evolution of the average number of part-time employed per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.39: Average Number of Dependently Employed in Crafts Occupations over the Period 2000–2008 Based on Administrative Data



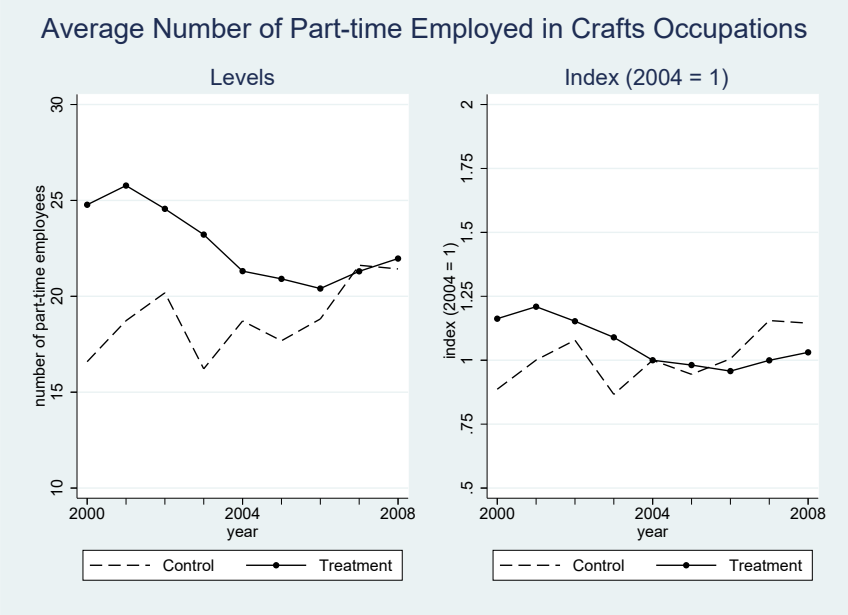
Data source: SIAB 2000–2008, own calculations. This graph shows the evolution of the average number of dependently employed (full-time and part-time) per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.40: Average Number of Full-time Employed in Crafts Occupations over the Period 2000–2008 Based on Administrative Data



Data source: SIAB 2000–2008, own calculations. This graph shows the evolution of the average number of full-time employed per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

Figure A.41: Average Number of Part-time Employed in Crafts Occupations over the Period 2000–2008 Based on Administrative Data



Data source: SIAB 2000–2008, own calculations. This graph shows the evolution of the average number of part-time employed per occupation in the treatment versus control occupations in levels (left panel), as well as relative to the base year 2004 (right panel).

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Selbstständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbstständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den Hanna Zwiener